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12. *The Glasgow Medical Journal*. Published Quarterly. Griffin and Co. (Received irregularly.)

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14. *The Dublin Medical Press*. Published Weekly. (Received regularly.)

#### INDIA.

15. *The Indian Annals of Medical Science; or, Half-Yearly Journal of Practical Medicine and Surgery*. Calcutta: Lepage and Co. (Received regularly.)

16. *Transactions of the Medical and Physical Society of Bombay*. Printed at the Bombay Education Society's Press. (Received regularly.)

17. *The Madras Quarterly Journal of Medical Science in all its Branches: including Original Essays, Reviews, Reports, and Medical Intelligence*. Madras: Gant, Brothers. (Received Nos. 1 and 11.)

#### AUSTRALIA.

18. *The Australian Medical Journal*. Melbourne: Buzzard. Published quarterly. (Received regularly.)

## AMERICA.

19. *The American Journal of the Medical Sciences.* Edited by Isaac Hays, M. D. Published Quarterly. Philadelphia: Blanchard and Lea. (Received regularly.)

20. *The North American Medico-Chirurgical Review.* A Bi-monthly Journal. Edited by S. D. Gross, M. D., and T. G. Richardson, M. D. Philadelphia: Lippincott and Co. (Received regularly.)

21. *The American Medical Times; being a Weekly Series of the New York Journal of Medicine.* Published Weekly. New York. (Received regularly.)

22. *The American Journal of Science and Arts; conducted by Professors Silliman and B. Silliman, Jun., and J. D. Dana, &c.* Published Bi-monthly. New Haven. (Received regularly.)

23. *The American Journal of Dental Science.* Edited by C. A. Harris, M. D., and A. S. Piggot, M. D. Published Quarterly. Philadelphia: Lindsay and Blakiston. (Received regularly.)

24. *Charleston Medical Journal and Review.* Published Monthly. Charleston, U. S. (Received regularly.)

## FRANCE.

25. *Gazette Médicale de Paris.* Published Weekly. Paris. (Received regularly.)

26. *Gazette Hebdomadaire de Médecine et de Chirurgie.* Published Weekly. Paris: Victor Masson. (Received regularly.)

27. *Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue des nouvelles, scientifiques, nationales et étrangères, &c.* Published Monthly. Paris: Labé. (Received regularly.)

28. *Journal de Pharmacie et de Chimie, &c.* Published Monthly. Paris: Victor Masson. (Received regularly.)

29. *L'Union Médicale, Journal des intérêts scientifiques et pratiques, moraux et professionnels du Corps médical.* Published three times a Week. Paris. (Received regularly.)

30. *La Lancette Française, Gazette des Hôpitaux civils et militaires.* Published three times a Week. Paris. (Received regularly.)

31. *Le Moniteur des Sciences Médicales et Pharmaceutiques.* Rédacteur en chef, M. H. de Castelnau. Paris. Published three times a Week. (Received regularly.)

32. *Revue Médicale Française et étrangère, Journal des Progrès de la Médecine Hippocratique.* Published twice a Month. Publié par le Docteur Sales-Girons. Paris. (Received regularly.)

*Archives Générales de Médecine; Journal Complémentaire des Sciences Médicales.* Published Monthly. Paris: Labé. (Received regularly.)

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**34. Bulletin de l'Académie de Médecine. Published Monthly. Paris: Baillière. (Received regularly.)**

**35. Mémoires de l'Académie de Médecine. (Received regularly.)**

**36. Revue de Thérapeutique Médico-Chirurgicale. Published twice a Month. Paris: Dr. A. Martin-Lauzer. (Received regularly.)**

**37. Journal de Médecine et de Chirurgie Pratiques à l'Usage des Médecins. Published Monthly. Par Lucas-Championnière. Paris. (Received regularly.)**

**38. Journal des Connaissances Médicales pratiques et de Pharmacologie. Published every ten days. Paris. (Received regularly.)**

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**46. Gazette Médicale de Lyon. Dirigée par le Dr. P. Diday. Published Bi-Monthly. (Received regularly.)**

**47. Journal de la Physiologie de l'Homme et des Animaux. Publié sous la Direction du Docteur E. Brown-Séquard. Paris: Masson. Published Quarterly. (Received regularly.)**

**BELGIUM.**

**48. Mémoires et Bulletin de l'Académie Royale de Médecine de Belgium, Brussels.**

**49. Annales D'Oculistique. Fondées par le Docteur Florent Cunier. Published Monthly. Brussels. (Received regularly.)**

**50. Annales et Bulletin de la Société de Médecine de Gand. Published Monthly. (Received regularly.)**

## GERMANY.

51. Zeitschrift für rationelle Medicin; herausgegeben Von Dr. J. Henle und Dr. C. v. Pfeufer. Published Monthly. Heidelberg and Leipzig: C. F. Winter. (Received regularly.)

52. Vierteljahrschrift für die praktische Heilkunde, herausgegeben von der medicinischen Facultät in Prag. Published Quarterly. Prague: Karm André. (Received regularly.)

53. Canstatt's Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern. Redigirt von Pr. Scherer, Pr. Virchow, und Dr. Eisenmann. Würzburg: Stahel. (Received regularly.)

54. Aerztliches Intelligenz-Blatt. Organ für Bayerns Staatliche und öffentliche Heilkunde. Munich: C. Kaiser. (Received regularly.)

## PRUSSIA.

55. Archiv für pathologische Anatomie und Physiologie, &c., Herausgegeben von R. Virchow. Berlin: G. Reimer. Published Monthly. (Received regularly.)

56. Archiv für Klinische Chirurgie. Herausgegeben von Dr. B. Langenbeck. Redigirt von Dr. Billroth, Prof. der Chirurgie, und Dr. Gurlt, Docent der Chirurgie in Berlin. Berlin: August Hirschwald.

## HOLLAND.

57. Archiv für die Holländischen Beiträge zur Natur- und Heilkunde. Herausgegeben von F. C. Donders, Utrecht, und W. Berlin, Amsterdam. Utrecht: Kemink and Zoon. (Received irregularly.)

## NORWAY.

58. Norsk Magazin, for Lægevidenskaben, udgivet af det medicinsk. Selskab i Christiania. Redigeret af W. Boeck, Faye. A. W. Münster. Lund: Voss. Published Monthly. Christiania: Feilberg & Landmark. (Received regularly.)

## SWEDEN.

59. Hygiea, Medicinsk och Pharmaceutisk Månads-Skrift. Published Monthly. Stockholm. (Received regularly.)

## DENMARK.

60. Bibliothek for Læger. Udgivet af Direktionen for det Classenske Litteraturselskab. Redigeret af Dr. E. Dahlerup. Copenhagen: Reitzels. (Received irregularly.)

## ITALY.

61. Bulletino delle Scienze Mediche. Pubblicato per cura della Società Medico-Chirurgica di Bologna. Published Monthly. (Received regularly.)

62. *Giornale Veneto di Scienze Mediche*. Published Monthly. (Received regularly.)

63. *Lo Sperimentale ovvero Giornale Critico di Medicina e Chirurgia per servire al Bisogni dell'Arte Salutare*. Direttore Prof. C. C. M. Bufalini. Published Monthly. Florence. (Received regularly.)

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Mr. Butcher on Excision of the Upper Jaw and Malar Bone

THE DUBLIN  
QUARTERLY JOURNAL  
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FEBRUARY 1, 1861.

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PART I.  
ORIGINAL COMMUNICATIONS.

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**ART. I.—*Reports in Operative Surgery.*** By RICHARD G. H. BUTCHER, Esq., M. R. I. A., F. R. C. S. I.; Chairman of the Surgical Court of Examiners, and Examiner on Surgery in the Royal College of Surgeons in Ireland; Surgeon to Mercer's Hospital; and Lecturer on Clinical Surgery.

1. SUCCESSFUL EXCISION OF THE ENTIRE UPPER JAW AND MALAR BONE, FOR AN ENORMOUS TUMOUR INVOLVING BOTH, AND FILLING THE PAROTID REGION.
2. SUCCESSFUL EXTIRPATION OF THE EYE, FOR TRUE SCIRRHUS OF THE EYEBALL.
3. SUCCESSFUL EXCISION OF THE ELBOW JOINT; NEARLY FIVE INCHES OF BONE CUT OUT; RECOVERY, WITH PERFECT USE OF THE LIMB.
4. SUCCESSFUL EXCISION OF THE ENTIRE RADIUS FROM ONE ARTICULAR SURFACE TO THE OTHER, WITH NEARLY ALL THE FUNCTIONS AND POWERS OF THE LIMB PRESERVED.

SEVERAL cases force themselves upon me for precedence in the present number of these "Reports;" and yet, though having a very short time ago written a memoir in reference to excision

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of the upper jaw, and again, more recently, dwelt upon various additional modes of removing the bone and portions of it, I again, nevertheless, select the subject for a first place, because it enables me to pay a passing tribute of homage and praise to the great surgeon who originated this bold project, and who has been recently taken from amongst us, happily according to the course of time; not ruthlessly torn away, but gently removed in old age, while yet in full and vigorous possession of his faculties and mental acumen, covered with distinction and honour. The finger of science, throughout time, must ever point to the name of Lizards, as conspicuous amongst the pre-eminent of the Scottish school.

**CASE I.**—*Successful extirpation of the entire upper jaw and malar bone, for an enormous tumour springing from the former, extensively implicating both, and filling up the parotid region. Mode of securing the patient. Advantages of the free application of the actual cautery. Mode of dealing with salivary fistula.*

Thomas D., aged 45 years, admitted to Mercer's Hospital March 9th, 1860. At this date he was recommended to me by a very able surgeon in the country, as a suitable case for operation. The man's appearance certainly presented very frightful deformity, owing to an enormous tumour, involving the left superior maxillary and malar bones, and filling up the parotid region, on the same side, implicating, far and wide, the important parts traversing the space. The suggested operative procedure of total removal was truly a formidable undertaking, a bold and terrible measure; but, as presently will be seen, practicable as suggested by my learned friend, and happily carried into effect. I shall first briefly detail the history of the case, its commencement and progress—the special operation, its intricate and complicated steps—the pathological states and conditions of the morbid product—and, finally, make a few practical observations, bearing upon the facts. Nearly two years before his admission, he was first apprised of the commencement of those changes which so quickly followed in succession—a dull, heavy pain, fixed in the upper jaw, under the floor of the orbit, and to the outer side of the canine depression; this soon changed its wearying character for one of a more acute and violent nature, coming on at quick and repeated intervals, and usually located in the site of the two last molar teeth. No arguments could prevent the man having the teeth drawn. They were removed, but with no mitigation, or even temporary rest, from suffering. "For six long

and weary months," as he expressed himself, did this deep and boring pain fix itself within the bone—often preventing sleep altogether, frequently impairing it for nights in succession. Yet all this time there was no swelling, no enlargement. However, quickly after this, in the seventh month, changes were being brought about, alterations manifest, and, in quick succession, they supervened upon each other; the hollow beneath the orbit began to disappear; quickly its depressed surface became on a level with the edge of the orbit and the alveolar range; soon again the bone was forced out, as prominently as the malar; and now the sufferings began to assume even a more aggravated type; pain diffused over a more extensive surface; located in organs—the eye, the ear; developed in contiguous surfaces, the nose, the palate, the throat. Thus the case gradually progressed, up to the time of his coming under my observation. His condition altogether was so remarkable, that I shall endeavour to map out all interesting particulars with as faithful and accurate a hand as so serious an investigation merits, nay, even demands. For several months after this time the pain became comparatively trifling, as contrasted with what it had been in the early development of the tumor, even though its magnitude was steadily progressive. Suddenly, however, as the tumor encroached upon the parotid space, his sufferings were again augmented. The appearance of the man, previous to operation, is accurately depicted in plate I. figure 1, from a truthful picture drawn by Connolly, and beautifully lithographed by Forster. It will be seen that the whole countenance was hideously distorted. The tumour now was fully as large as the two clenched fists; springing from within the antrum, it forced out its walls in every direction, impinging on every neighbouring cavity. Its outward direction being most palpable at first, but soon passing mesially, it forced in the nasal partition, thrusting gradually aside the turbinated bones; blocking up altogether the left nostril from the anterior to the posterior opening, and slightly bulging the septum nasi to the opposite side; it did not protrude in front, and only to half an inch behind, being in both these aspects firm and dense to the touch, not yielding blood, even on forcible pressure. By its volume, too, the palate-plate was thrust down upon the tongue three-quarters of an inch below its normal position, and here also the same solid characters marked the growth. There was nothing peculiar in the colour of the part, save that it was of a darker hue, and probably a little more vascular than the same region on the healthy side. This deeply depressed state of the palate did



not compensate sufficiently for the rapidity of the growth and the expansive force exerted by its progress; for the orbital plate of the maxilla was bulged upwards, partly dislocating the eye from its normal bed, altogether from its proper axis, at the same time everting the organ, and making so tense the optic nerve, that the vision of the eye was exceedingly imperfect. On examining with the finger, this change in the bony plate could be felt. We next come to consider the tumour in its anterior and external aspects, involving, as it did, the whole side of the face, from the nasal process to the ear, and particularly from the junction of the malar and frontal bones to the angle of the jaw. The measurements here passed over an enormous projecting mass, so that the integuments were borrowed from neighbouring parts—the nose dragged from right to left, and the mouth in the same direction; the lips slightly apart, and the jaw depressed; being kept so by unnatural encroachment of the growth within acting as a mechanical obstacle, and to a somewhat greater extent in acquiescence to the will of the patient, to admit a free supply of air, in compensation for the obstructed nostrils. Following the tumour outwards, it was found to have incorporated with it the malar bone, throughout its entire extent; and, on opening the mouth, the tumour was found to pass backwards and outwards, blocking up the entire pterygo-maxillary fossa, and projecting beneath and behind the angle of the jaw; while a more superficial part passed externally to the jaw, matting in its surface the structure of the masseter muscle, and becoming imbedded in the parotid. The deep and intricate relations of the growth in this region were alarming. Yet, withal, there was one hopeful feature even in this direction—the same solid character stamped the tumour; in other words, it could, by careful manipulation, be definitely followed by insinuating cautiously the tips of the fingers; at least *thus* I was satisfied in my own mind. The entire expression of the man was marked with grief, and very pitiable. The integument covering the tumour presented throughout nearly a natural appearance; over the most prominent parts of the growth, however, it was of a dusky-red colour, evidently from congestion, owing to pressure from behind retarding its circulation, and probably acting on a larger supply of blood than had been destined for its supply; it was likewise thinned and shining, yet movable upon the tumour; not identified or incorporated with it at any point. The patient permitted the tumour to be handled with impunity; and no pain was elicited during the manipulation, or lurked in it after. To the touch, it was braced, firm, and unyielding

throughout; perfectly immovable from base to apex; and, in all its characters, affording a fine example of osteo-sarcoma.

The man, though afflicted with this formidable tumour, seemed otherwise in good health; and as he pressed strongly that it might be removed, I saw no valid reason against complying with his request. On the 14th of March, 1860, I operated after the following manner. Before, however, entering upon the several steps of this difficult measure, I shall digress for a few moments, to dwell upon the way in which the patient was secured—precautionary measures of the kind not being sufficiently attended to. The man being seated in a strong, heavy arm-chair, made of oak, his feet were raised a few inches from the ground, and lashed to the corresponding legs of the chair, while his arms were secured to it laterally and behind. To prevent any upward movement, a strong slip of wood, about four inches wide, was passed under the arms of the chair, and across the thighs. I would wish to insist very strongly on these directions; because, speaking practically, I have seen before now, the patient, when not properly secured, break away from all restraint made by hands, and only by force dragged back to the position which he should never have been permitted to quit. It seems a cruel act to lookers-on, the preparatory measure of tying the patient; but it is a practice that never should be dispensed with. No surgeon can estimate beforehand the power which his patient may possess of enduring pain; and no patient can be so satisfactorily cognizant of the fortitude he may command, as to resist a goading agony, that, in its intensity, he never could by possibility have even imagined; and, when felt, the all-powerful impulse to burst away and escape is that alone which takes possession of the sufferer. But even the remote likelihood of such a casualty, granted the term to those sceptical, should never be risked: the patient should be so protected as to be incapable of injuring himself. This precautionary security I hold to be the duty of the surgeon to enforce—nay more, it is an extension of leniency to the individual that is to suffer; for by its adoption, rapidity of procedure is facilitated, if intricate dissections are to be executed, the patient's safety is insured; all suffering is abridged; and, when chloroform is inadmissible, the shock of operation is lessened in its duration. A few words as to the support for the head of the patient. I greatly prefer it resting upon the breast of an assistant, to being sustained against a high-backed chair; because facilitating movements in various directions, and for several purposes, viz., the more ready application of instruments, often made to act more effectively by

ever so trifling a change in the position of parts; the facility afforded where cautious and minute procedure is called for, the patient not being able to dip down and droop the head; and, finally, as being more under command to clear out coagula, to facilitate the application of cauteries, the ligaturing of vessels, and the arrest of hemorrhage. It may appear superfluous to enter into these minute particulars. To those accustomed to deal with such severe measures as I am about to detail, they will indorse these precautionary measures with their sanction. As to the man who reads these pages, wrapped in self-sufficiency and egotism, I have nothing to say. But I would speak faithfully, so as to guide the young surgeon; and I think I would be culpable if I did not append these cautions, because, if followed out, acted up to, they will, as I conceive, be powerful accessories towards establishing a confidence and a self-reliance in the operator; and materially tend towards securing a favourable issue, when everything otherwise would seem hopelessly beyond the pale of operative surgery.

The patient thus placed, with a strong light upon his face, I passed a sheathed bistoury into the mouth, its blade being directed from the left commissure towards the malar bone, and its point steadily fixed against the cheek, as high as the connexion between it and the tumour would permit of. The sheath was then withdrawn, transfixion accomplished, and the mouth laid open from within, outwards, along the track of the instrument. A quick dash of blood might have been expected from the facial artery to follow this stroke of the instrument; but the precautionary step of compressing the vessel while bending round the bone was effectively executed in preventing such a loss, until its cardiac end was ligatured. This incision did not at all reach high enough, so the bistoury was laid aside, and the incision prolonged with a scalpel, over the tumour, up to the junction of the frontal and malar bones. The knife was then carried backwards and outwards along the zygomatic arch as far as its tubercle, or junction of the two roots. The flaps thus marked out were adherent and deeply implicated with the prominent development beneath. I next proceeded to dissect up the superior flap from the tumour, commencing in front, and detaching with it the left ala of the nose from its bony connexions. With cautious sweeps of the knife, the parts exterior to this likewise were freed, the blade of the instrument being kept close to the bone; and, while it passed outwards, the infra-orbital nerve was cut, where it emerged upon the cheek; and thus, too, the vessels passing through the same canal. As the knife travelled outwards, great caution was requisite to dissect the flap

off the tumour, without breaking its integrity, the integuments were so strained and thinned from the inordinate pressure exerted by the morbid growth proceeding from within. However, this being satisfactorily accomplished, due care was necessary in lifting the entire flap by cautious touches of the knife, still upwards, so as clearly to reveal the lower segment of the osseous margin of the orbit, and even still further the forced-up plate, constituting the floor of that cavity. This dissection was delicate and complicated, because threatening the distorted organ of vision. The lower flap was next attacked, commencing at the angle corresponding to the outer wall of the orbit, and dissected downwards and backwards. The separation of this covering from the tumour was difficult in front, for the same reason as assigned when dealing with the upper flap; and as the dissection was carried back, the whole parotid region was exposed, being filled up by the excessive growth. The lower flap had to be set free, thrown down considerably below the angle of the jaw, before its superficial outline could be revealed in its entire extent. The man now got copious draughts of wine; for though no blood was lost, yet the shock manifestly produced a very decided effect, though these proceedings were executed with all the rapidity with which the knife could be made safely to travel.

Now that the entire mass of disease was fairly exposed, it was most pleasing to dwell upon the accuracy of the diagnosis, as to the amount of osseous parts implicated, and which must be taken away. It had been surmised, but was now revealed by demonstration, that the entire left maxillary bone, together with the malar, with their several and wide-spread processes, were all hopelessly seized upon, made parcel of, and distorted in its increase.

When the flaps were drawn apart, the superior thrown upon the forehead, the inferior hanging pendulous over the left side of the neck, with the projecting massive tumour between, an appearance was presented sufficiently alarming to cause several of the students to grow sick and faint.

The next step in the operation was the division of the hard parts. By doing so at this stage of the dissection, I was confident I could more easily get at the external and parotidæan encroachments of the tumour. To accomplish this end, I drew the incisor tooth on the left side, and then passed one blade of a long narrow bone forceps into the corresponding nostril back into the pharynx, the other into the mouth. In a second the section was completed. The nasal process of the maxilla was next clipped across at its junction with the frontal

bone. The forceps was next applied to the orbital margin, at the junction of the malar and frontal bones on the outside, and the division made into the spheno-maxillary fissure. The zygomatic process of the temporal bone was then cut across, just in front of its tubercle; the forceps was then laid aside. Spreading the end of a towel upon the tumour, so as to prevent the hand slipping, I seized the maxilla, together with the growth, getting my thumbs well in upon the dense orbital margin, and broke down the entire from its posterior attachments. The detachment of the nasal, orbital, and palate plates, was all that could be desired; and then, by a few semicircular wrenches, and breaking down of parts, by the index finger passed far back, all connexions were safely set free, even into the pharynx; and the wrenching of the parts lacerated the vessels so, that no bleeding worth making mention of followed the proceeding. This was all very well, as regarded the anterior and mesial attachments of the tumour; but its most dangerous relations were deep in the parotid space. All the superficies of the growth, as has been already mentioned, was fully exposed; but it required a long, painstaking, and careful dissection, to set it free from the important parts lodged in this region. Behind, the growth covered the external carotid, which was pressed somewhat backwards, and deeper than its normal course, while to its internal side lay the carotid for the brain, in its upward course; in connexion with their primitive trunks passed their corresponding veins, while, surrounding it on all sides, were vessels of magnitude, and nerves essential to life. The division of the several fixed or bony points rendered the tumour somewhat movable, and the crushing of its holdings behind, as before mentioned, increased this mobility,—circumstances of the greatest moment, as facilitating the tedious and dangerous dissection throughout this difficult proceeding. The knife's edge was kept well to the tumour; and wherever it was hard, or firm and smooth, the handle of the scalpel—the end of the index finger better than all—pressed away slightly adherent parts from it. Thus, by cautious manœuvres, by occasional touches of the knife, by breaking down adhesions, by twisting and wrenching with a guarded force the growth from its bed, the entire was brought safely away. But little blood flowed during the outer and posterior dissection; a couple of large vessels had to be ligatured, but not one on the inside. On carefully examining the extensive chasm, not a particle of the tumour could be detected as being left behind; and a similar inference was arrived at from an examination of the specimen removed. I next cau-

terized with the hot iron the entire raw surface within. And now I would wish to say a few words in reference to the use of the cautery. My impression is, that it is not used as much as it should be in operations about the mouth. In all instances, whether there be hemorrhage or not, whether the growth be suspicious as to its characters of malignancy or not, I invariably use it, and that freely. If there be hemorrhage, its application arrests the flow; if parts are suspiciously malignant, it destroys contaminating radicals,—tendency to recurrence; if no bleeding, or malignancy, its touch quickly arouses an inflammatory action, healthy in its character, vital in repair. I have no doubt, by this rapid, healthy change, destructive, diffuse, erysipelatous forms of inflammation are guarded against. During the latter stages of the operation, the patient was supplied with brandy, and demanded it with a liberal hand.

All oozing of blood from the parts having ceased, the flaps were adjusted, and held together by several points of the interrupted and twisted suture. The former answered very well where the integuments were thinned, and at the angle above and throughout the horizontal wound; while the latter was most serviceable and efficient at the angle of the mouth, and again about three-quarters of an inch above this point, where the thickness of the parts was not altered or changed. The sutures were sufficiently numerous to hold every portion of the flaps in juxtaposition, sticking-plaster not being at all applicable, owing to the flaccidity of the detached parts. After the tumour was removed, the eye receded from its forced position, and drooped; and, I need scarcely remark, the lids remained open, from the temporary injury inflicted on their nervous supply. The organ was, however, supported in its natural position, in its proper axis, by compresses of lint passed into the chasm beneath, each having a silk cord attached, brought out of the mouth, and secured upon the forehead. By this mode, too, the cheek was prevented from falling back as much as it otherwise would have done. The patient being now sufficiently restored from shock, he was removed, in the horizontal position, from the operating theatre to his bed. The bed was made comfortable for his reception, being heated by jars filled with boiling water; the increased temperature was most grateful to his cold feet and chilled surface. He lay upon the affected side, with the head slightly raised, which position allowed the increased saliva—the weeping of fluids from the cut surfaces—to flow readily from the mouth, without irritating the parts concerned in respiration or deglutition.

The exterior characters of the tumour having been de-

scribed, I shall say a few words as to its internal arrangements upon section.

The results of the morbid inflammatory action were everywhere evident upon the walls and within the maxilla—thickening, condensation of new osseous matter, in all its several changes, from earliest deposition to the completion of perfect bone, thickening of periosteum, enlarged blood-vessels. In other points, bony structure—even of original formation—thinned, and flexible, and elastic, protruded by sarcomatous depositions from behind, characterized the growth; in some places, masses of broken down gelatiniform matter abounded; while in other localities, the deposit was cartilaginous, springy, and tough, like india rubber. In a few positions, the cancelli of the bone, particularly in the body of the malar, were as it were distended, forming numerous chambers, filled with morbid matter; while, pervading its external parts, numerous osseous spherules were developed, some adherent and protruding out from the solid bone wall; while in other localities, detached pieces could be found in cartilaginous beds. Softened patches might likewise be observed, giving much the resemblance of encephaloid cancer; yet, on being subjected to microscopic examination, no such degeneration could be verified as yet to have taken place. I not only examined the growth carefully myself, under the microscope, with regard to the all-important question of malignancy, but I submitted several portions of it to Dr. Mason, the able lecturer on Physiology. His report confirmed the conclusions I arrived at, namely, its non-malignant nature.

In two hours after the patient was put to bed, there came on some slight weeping of blood; but it ceased shortly, of its own accord, being so trifling as not to call for interference. It was, as I have elsewhere mentioned, occasioned by *the first injection of divided vessels by the exalted circulation of reaction*. In six hours after the operation, the cheek was warm, heat well generated over the body, and the man sleeping quietly.

On the following morning, 15th, the report was very favourable: he had a quiet night, slept, took freely of cold chicken-broth, and some wine. The cheek remains well sustained; the eye steadily supported in position, though the lids are still wide apart, motionless, and the conjunctiva a shade vascular; at the same time, the eye dull, its brilliancy lessened.

16th.—Had an excellent night, very little suffering; partakes freely of milk, eggs, beef-tea, and wine.

17th.—Slept quietly, and has partially regained the power of closing the lids; some swelling over the parotid region;

support as before, and a succession of warm stupe-cloths to the inflamed part.

On the 19th I removed the superficial plugs, leaving in the two immediately beneath the eye. Stupes beneficial in depressing the swelling, noted on yesterday; to be continued.

20th.—Removed the needles, and some of the stitches; union of the cheek perfect throughout, particularly in the thicker parts. Suffered the threads to remain where the covering of the tumour was thinned, as the bond of union, though accurate, was yet weak. Nutriment and support in abundance.

On the 22nd, cut out the remaining stitches; union perfect from one end to the other of the incisions. Removed also the remaining plugs; they came away with facility, being coated with purulent matter; and they fulfilled their office well, as the eye retained its natural position, being evidently held up by recent attachments, brought about, completed while the organ was steadied by this artificial support. At this time, too, full control was obtained over the lids; and the voluntary movements were effective in restoring brilliancy and tone to the cornea and its covering.

On the 7th April, I opened a small abscess in the parotid region, or rather in front, over the masseter muscle. From this flowed out pus and saliva in abundance, the latter fluid continuing for some days to drain freely from it. I passed an armed probe with a few threads of silk from the external opening, through the abscess, forwards into the mouth, and tied the seton up; and in a few days, having established a channel for the saliva into the mouth, I pared the edges of the external wound, brought them together, maintained them so by two points of twisted suture, using very fine needles, and in three days the wound was healed and closed permanently, and the saliva continued to pass by the artificial canal into the mouth.

On the 10th, the man was up, and walking about the ward. The cheek healed accurately throughout, somewhat flattened, being slightly corrugated about its centre; but the eye remained admirably supported, and following its fellow in all its motions; and the lids retained their place, and performed their functions as if the parts had never been meddled with. (See Plate I., Fig. 2.) On looking within, the surfaces are all covered with healthy granulations, and almost finally repaired. The functions of deglutition are well performed, and every day he is obtaining the power more and more of eating solids, and swallowing the bolus without hesitation; while fluids pass backwards without the slightest regurgitation through the



nostrils, His speech, too, is becoming again distinct, and he finds no difficulty in expressing his wishes.

A few days later and the man was up, and walking about the ward; and at the end of six weeks from the date of operation, he was sent home to the country perfectly cured.

I shall not leave this subject without directing the reader's attention, which I do with great pleasure, to an able paper on "Removal of the Upper Jaw," by Dr. Z. Johnson, Surgeon to the Kilkenny County Infirmary, and published in this Journal\*, in which he gives the full details of a most interesting case operated upon by himself; and I must say the whole proceeding redounds greatly to his credit, judgment, and skill.

There are several interesting points in the case which I have detailed, for the practical surgeon to contemplate and dwell upon. It teaches the absolute necessity for accurate diagnosis, so that tumours be not meddled with, implicating the base of the skull, and perhaps incorporated with the very membranes of the brain, as I have seen them. It teaches how the surgeon must adapt the line of his cutaneous incisions according to the direction of the bulk of the tumour. It gives a confidence, how that the eye, though distorted, thrust out, and interfered with in its functions, may again be restored, maintained in position, and bring back enjoyment by its recovered powers. Finally, by the successful result, confidence is gained, and operative surgery maintains its exalted position.

**CASE II.—*True scirrhus of the Eye-ball, occurring as a primary affection, successfully extirpated; with special observations, particularly in reference to the operation.***

True scirrhus of the eye-ball is so exceedingly rare, as a primary affection, that I am gratified at having an opportunity of inserting this very remarkable case in these Reports in Operative Surgery. So seldom has it been met with, that the existence of the affection was doubted by Mr. Wardrop. In his most elaborate work on "Fungous Hæmatodes," he absolutely denies the existence of this affection. At page 87, he says,—“ I much suspect whether *cancer* ever affects the globe of the eye in its *primary* form; at least, I have never met with an example of this kind.” And again, a little further on,—“ I have never been able to obtain an accurate account of a single case, where any of the coats or contents of the eye-ball were the primary seat of cancer.” So, likewise, we have no

\* Dublin Quarterly Journal of Medical Science, August, 1858.

mention made of the affection in Mr. Tyrrell's book. Mr. Lawrence, in a Clinical Lecture, published in the "*Medical Gazette*," July 2, 1847, gives a brief description of a case, and refers to the specimen in attestation of the subject. I shall briefly give the details in reference to it, and then dwell upon the remarkable facts in relation with my own case. The specimen was removed from a middle-aged man by Mr. Wormald, and deposited in the Museum of Bartholemew's Hospital. It is thus described in the Catalogue of the Museum (Series ix., No. 17):—"The tissues of the anterior and inferior third of the right eye are occupied by an irregular growth of firm and very vascular substance, with a granulated, warty, and very vascular surface. The optic nerve, of which a portion is preserved, is sound. There was no return of disease in the orbit, but the patient died with medullary tumours in the heart and in some other parts, two years after the extirpation of the eye." The condition of the heart is thus described in the Catalogue:—"Part of a heart, in which there is a large mass of firm medullary matter in the substance of the apex of the right ventricle and of the septum. The morbid mass has not altered the external form of the heart; but it projects with a coarsely granular surface into the cavity of the ventricle, and has raised up the tricuspid valve; in its middle, its substance is softened and broken down. The pericardium is in every part closely adherent. The aorta is dilated, and both it and its valves have earthy deposits in them". The opinion of so eminent and so distinguished a surgeon as Mr. Lawrence upon this specimen should not be omitted, as we would submissively bow to his judgment in many cases; so I shall declare his opinion here. Referring to case No. iv., in his paper, and that which I have transcribed, he writes:—"Cancer of the eye is a rare affection; very few instances have come within my observation. The preparation now before you shows that the disease is less disposed to extend to the surrounding parts than when it occurs in many other situations: the female breast, for example. The anterior segment of the globe is converted into a dense scirrhus mass, of considerable thickness; while the posterior surface and the optic nerve are unaffected. The palpebræ, with their mucous membrane, the portions of muscles of the globe and optic nerve left in the orbit, were removed after death, and are seen in this preparation, free from disease. Thus, there was no return of disease in the part; but the patient died of secondary cancerous affections, having survived

\* *Op. cit.*, Series xii., No. 60.

the operation two years. It is worthy of remark that, although the primary affection was *genuine* scirrhus, the deposit in the heart had more of the medullary character."

And now with regard to the case which was operated on, in Mercer's Hospital, by myself.

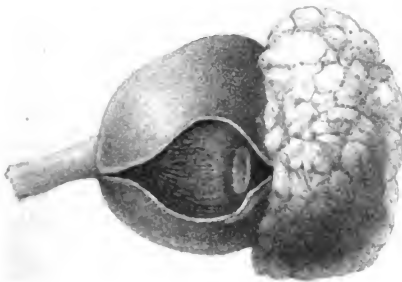
Eliza Doran, aged 69, admitted to Mercer's Hospital, November 1, 1859. Two years before the above date, she was suddenly seized with racking pain in the eye, which awakened her from sleep. On the following morning, the entire organ seemed red, with a profusion of tears flowing from it, which she ascribed to the severe rubbing and pressure that she made upon it to try and get relief from the burning pain in the ball. This pain remained unabated for days, and gradually the sight became dim; she could, for nearly three months, discern objects, but could not clearly distinguish their characters; gradually even this power of imperfect vision was lost; and in five months after the first occurrence of the pain, "a red fleshy pimple" came upon the cornea, and gradually, slowly, increased up to the time of her admission. Her condition, when submitted to my care, was as follows (see Plate II., Fig. 1):—the tumour was fully as large as the section of a walnut, but considerably more prominent; it projected between the lids, and their conformation imparted to it the oblong shape in the transverse axis. However, on separating the lids, the tumour then presented a more circular shape, and was about the size of a two-shilling piece; the growth at its base was considerably less, and seemed to exceed but in a very little way the imbedding of the cornea in the sclerotic coat; from this comparatively narrow base, then, it sprung, and spread out, overlapping to some extent the sclerotic, and projecting between the lids; the entire surface of the growth was mammilated, and made up of firm, dense, fleshy-coloured structure, which, to the touch, was hard as cartilage, did not bleed on being handled—in other words, of strictly scirrhus nature, with a thin watery secretion pouring from it and the conjunctival membrane on either side in great abundance. Pressure with the finger did not pain or hurt; the growth could be handled with impunity, though the wearying lancinating pain seldom ceased; and the exasperated hemi-crania, aggravated during night, seldom during these hours was remittent.

Accustomed to meet and deal with such matters, no doubt existed in my mind as to the nature of the case being true scirrhus, set up in the cornea and its conjunctival membrane, and developing itself to the remarkable size mentioned, without involving surrounding parts; for, absolutely, the marginal

*Fig. 1.*



*Fig. 2.*



W. Butcher on Extirpation of the Eye

*London: J. Smith, 1850.*



circumference of the cornea to the sclerotic was free from all apparent implication. This was quite clear, demonstrated on lifting upwards, and gently drawing forwards, the morbid product, the lids being restrained apart; all behind this marginal ring seemed exempt from disease. By passing the fingers gently and deeply into the orbit, a certainty of all the contained parts within this bony recess being uncontaminated was conclusively arrived at; the lachrymal gland, buried in its own recess, was free from enlargement, or centered as the nidus of pain. Those darting pains frequently passing through the brow, or those stings, habitual in the tumour, were never transferred to this special and suspicious locality. By the most careful examination I could not detect any other organ implicated in disease, and the woman readily assented to my proposal of removing the part. On the 11th of November, 1859, I proceeded to operate, after the following manner:—the patient was placed recumbent on the operating table, the head being raised by pillows; she was quickly brought under the influence of chloroform; the left eye being the one affected, the head was slightly turned to the left side, so that the blood, issuing from divided vessels, might have a ready drain outwards, and not obstruct careful inspection throughout the after steps of the operation. The head being carefully steadied, the upper and lower lids held apart by approved retractors, a curved needle, armed with a strong silk thread, was passed through the eyeball, or rather, anterior to that part from whence the malignant growth seemed to spring, thus traversing its base from without inwards, from its temporal to its nasal side, avoiding injury to the parts behind, so as to admit of clearer investigation afterwards as to the parts from which the growth had its origin and sprung; the ends of the thread being knotted, the needle was cut away, and thus a means secured of drawing the eye in any direction that might be required, so as to facilitate and expedite the steps of the operation. This being effected, the upper and lower lids were held apart by the retractors, and an incision carried outwards for three-quarters of an inch, disuniting the lids at their external commissure; the upper lid being elevated, and made tense to the degree required, and the eye steadily drawn forwards, I passed a fine long-bladed knife in a semi-circular sweep from the inner to the outer wall of the orbit, freely dividing the conjunctival membrane, and carefully the levator palpebræ muscle, and the muscles attached to the ball, throughout this trajet; so likewise the knife was swept in the under corresponding range; a gentle touch on the inside liberated the organ, while a more bold and, at the same time,

scooping evolution of the knife, brought down the lachrymal gland entire from its well-marked depression; the blade of the knife being long, and made in each evolution to move according to the conformation and axis of the orbit, it will be readily understood how efficiently all the external parts were divided and set free; blood flowed freely throughout these incisions; but, from the admirable position of the patient, it did not interfere with quickly prosecuting the end aimed at, the perfect extirpation of the organ by section of the attachments of the recti muscles, and division of the optic nerve and its accompanying vessels far back. This was readily achieved by passing the knife along the outer wall of the orbit well back, the eye being gently drawn forwards and inwards, so as to make tense the parts; by a sudden decided stroke from above downwards, the muscles yielded, and the nerve was severed, and so the parts liberated came readily away; blood flowed briskly, but was restrained by passing a few strips of lint into the chasm, so as completely to fill it up, and by then laying down the lids, and placing a compress outside all, retained by the gentle pressure of a few turns of a bandage; the lids were not stitched where separated at the outer commissure, for reasons presently to be noticed; she quickly recovered from the chloroform, and was conveyed to bed; shortly after an opiate was given, and sleep procured. It is unnecessary to give the daily reports; the patient went steadily on to cure.

On the third day after operation, one of the strips of lint was gently withdrawn, and on the fourth the two remaining stripes; no blood flowed after. The orbit was gently syringed out with tepid water once a day, and a little simple ungent smeared upon the lids, where they came in contact. Steadily the purulent discharge diminished, and ultimately ceased altogether. Sedatives were given to procure rest, and the most liberal diet from the time of the operation to her perfect recovery. She left the hospital on the 5th of December. It was most remarkable the great change brought about in this old creature by so successful an operation; her nights were undisturbed; her appetite was restored, and quickly she gained strength, put up flesh, and never ceased to reiterate her prayers and thanks.

Independent of the external characters, so typical of malignancy, and so readily recognised by the practised eye, the most careful microscopic examination of the tumour led to no other evidence, than the true scirrhus nature of the growth. The characters revealed by the microscope were very analogous to the representation annexed to the case, No. 5, one of the many

figured in an elaborate paper which I published in this journal, "On Encephaloid Cancer, and the Cancerous Degeneration of Warty Excrescences, with the operative treatment applicable to each". To place the question beyond all doubt, I submitted a part of the tumour, for microscopic examination, to Dr. Mason, the accomplished microscopist, and lecturer on physiology, in the "Original School of Medicine," Peter-street; his account perfectly harmonises with the conclusions arrived at by myself, and in every way confirms the accuracy of the investigation. On prosecuting the dissection with the knife, it was very evident that the entire structures, the whole thickness of the cornea, were matrixed in the abnormal production; and here, again, the microscope substantiated this conclusion. On removing the adherent fat and areolar tissue, and elevating, at the same time protecting carefully, the attached insertions of muscles, all the globe posterior to the encroachment of the morbid growth seemed on the outer surface healthy; the bulk or volume of the part was not augmented; and the sclerotic coat, throughout, presented its normal glistening appearance; the optic nerve was healthy in its section, natural in its form, and unaltered at its external conformation. After the foregoing conclusions were arrived at, from the most careful and repeated examination of the recent parts, then the specimen was plunged into proof spirit, so as to prepare it for an adequate and just examination of the parts within. After five weeks elapsed, it was then carefully examined; and the following conditions testified to the effect of the spirit, so as to render very satisfactory the examination: the globe of the eye, posteriorly, presented its full convexity, and the sclerotic was, as it were, stiffened; a section of it was made with a sharp fine knife, from behind the malignant growth, along its upper surface, and backwards, to within a few lines of the entrance of the optic nerve; its structure was neither thickened nor thinned from its normal state, anteriorly or posteriorly; by separating the edges, a strict and beautiful view of the parts within was afforded; the posterior chamber was not encroached upon in any way; the retina lay as a whitish thin film in plicæ, or folds, in the bottom and towards the lower part of the sides of the chamber, and was natural in every respect, as well as the nerve at its entrance expanding into it. On floating the specimen in spirit, and gently agitating it, the plicæ unfolded, the delicate mesh waved through the fluid, unchanged and unaltered, save by condensation with the spirit. The chamber was not altered in its walls, the ciliary processes

\* Dublin Quarterly Journal, November, 1856.



were healthy, neither was it encroached upon from before; for the lens lay in its natural position, while the iris was resting against it, but not adherent; and the anterior chamber was obliterated—the cornea, as already mentioned, being not only incorporated, but forced backwards, flattened towards this feeble partition. (See Plate II., Fig. 2.)

On the most careful examination, there was no evidence of contamination beyond the limits already assigned; the strictest microscopical investigation was pursued of every adherent or approximating tissue, the muscular, areolar, &c., the lachrymal gland,—but all seemed exempt from contamination, as might have been with certainty prognosticated from the isolated manifestations of this rare affection.

In Mr. Lawrence's remarks, published in the *Medical Gazette*, as already quoted, the similitude of the rare specimen which he exhibited to the class bears a most remarkable analogy to mine. See how strictly the description harmonises with what I have written. Now, whether the pathological condition of the cornea described was secondary to the involvement of the conjunctival membrane, I know not, as not having seen the case sufficiently early to determine. I am aware that Scarpa lays the seat of the true carcinoma as being the conjunctival membrane<sup>a</sup>; and Travers expresses himself, likewise, as holding the same opinion<sup>b</sup>, and to this effect; so far as present evidence extends, if we except the lachrymal gland, this *membrane* is the only texture connected with the eye, ever primarily affected with carcinoma.

Now, I conceive it necessary to write upon a few points relative to the operation:—1st. On all accounts, it is better to place the patient recumbent, and leaned a little towards the affected side, so that chloroform may be administered to the best advantage, and the safety of the patient; and that the blood may have a ready escape, and not interfere with the after-proceedings of the operation. 2nd. That the lachrymal gland should be taken away, in all instances, when the operation is for cancer, or when an artificial eye is not applicable, so that no undue secretions may be produced, streaming for ever over the cheek, the delicate lachrymal puncta being probably obstructed, spoiled in their functions, by the resenting inflammation set up by injured parts; this objection to its being left being altogether apart from its liability to secondary infection. 3rd. It is of great importance to the humble sufferer that has

<sup>a</sup> Scarpa on Diseases of the Eye, page 526, Second Edition.

<sup>b</sup> Travers' Synopsis of the Diseases of the Eye, page 90.

been compelled to seek safety for life in this shocking operation, that no disgusting traces should remain, by the lids being apart, as so frequently occurs, with a rather prominent red fleshy protrusion from within; or, if not projecting, at least appearing sufficiently repulsive within the space. Again, when all has been scooped out, the lid retracts, and a dark chasm appears below it; or, again, it shrinks deep back, with the under lid, possibly swollen, vascular, and painful, irritated by excitants from without; to the poor the numerous advantages of artificial eyes, even when applicable, are denied from sheer costliness and expense; in many, even the peculiarities of the case forbid their adoption; yet it is an all-important consummation to avert pain and suffering, to remove revolting deformity. Amongst the wealthy, too, sometimes, art purchased by money cannot separate the afflicted sufferers, in their sad lot, from the poor, as when the artificial eye is not applicable; and then each rests upon the same vantage-ground, the hopes and trust for relief from agony.

Now, when the artificial eye cannot be made applicable, be the sufferer rich or poor, it is of the utmost importance that extraneous matters floating about in the atmosphere, and the sharp winds of a changeable climate, should be guarded off from the delicate and sensitive parts within. I therefore suggest for adoption the method of managing the lids pursued in the foregoing case—namely, after separation of them by section at the outer canthus, and after the several steps of the operation to the removal of the organ, not bringing the divided surfaces in contact by adhesive straps or stitches, but permitting the upper lid to slightly overlap the lower, by a little more than the depth of its tarsal cartilage; if, as the wound heals, the drooping should threaten to be too great, then the lid may be slightly elevated, which can be readily effected by gently drawing it outwards, and maintaining it at the required tension, either by strap or stitch. Sometimes the lid, if redundant, will be inclined to fall inwards; this, too, may be prevented by the insertion of a stitch judiciously placed outside, so as to make a little tense the lid, and, at the same time, give it the least possible tendency to eversion of its tarsal edge; when the wound heals, this will not be found too great. By this method I have secured the most perfect immunity from annoyances by extraneous matters; by this mode I have secured the most placid expression of countenance, free, in every respect, from any repulsive or deformed trace.

The foregoing case is, I think, replete with interest, in many particulars, not alone from the extreme rarity of the affec-

tion, true scirrhus, generated, set up in the cornea and its conjunctival membrane, attacking the ball of the eye, an isolated manifestation of malignant disease; but, again, from the pathological conditions revealed by microscopic investigation, by the most careful dissection, bounding its encroachments with the nicest exactness; and, lastly, and above all, the important and consolatory evidence adduced from well-applied and timely interference.

When particular modes of treatment are brought into practice, and insisted upon, extolled, and based firmly on good grounds, it becomes the duty of those engaged in the work to sedulously guard the superstructure—at least, to endeavour to point out carefully what ought to be done in perfecting the work, not only by precept, but example; therefore I make no excuse for again recurring to excision of the elbow-joint; and I care not if I should be considered tedious, if the facts arrived at be at all proportionate to the deep value I set upon their just appreciation—even one truth realised towards the advancement of operative surgery.

The following case of excision of the elbow-joint illustrates many points of great interest, and forms an appropriate sequel to the papers which I had before the honour of publishing on the subject in this Journal\*:

**CASE III.**—*Successful excision of the elbow-joint and adjacent ends of the bones, five inches in all being removed. Great disorganization of the surrounding tissues, yet healthily restored. Recovery, with the motions of the limb nearly perfect.*

M. Reynolds, aged 20, was admitted to Mercer's Hospital, July 16th, 1859, with incurable disease of the left elbow-joint. Ten months before, he got a slight wrench of the joint, the pain and soreness after which disappeared; however, a fortnight had not passed by, when the same annoyance again recurred; he bore with it patiently, and doctored it at home with stupes and herbs; but, matters grew gradually worse, and at the end of the second month he lost all power in the forearm; he could neither flex nor extend it, though retained in a state of partial flexion, with the hand pronated; the swelling gradually increased, the pain became more intense; he had several opinions, and various modes of treatment put in prac-

\* On Excision of the Elbow and Wrist-Joints, and the Preservative Surgery of the Hand, Dublin Quarterly Journal, 1855; and Reports in Operative Surgery, Dublin Quarterly Journal, February, 1859.

tice for its restoration, but all to no effect; for a time the intolerable suffering would be alleviated, and the high fever subdued; but still each accession and repetition of active inflammation added to its bulk, augmenting the proportions of the limb, even above and below the joint. After the fourth month, abscess formed, and broke by an ulcerated patch above and to the inside of the internal condyle. Others, at a later period, formed and burst on the forepart of the limb. Each suppurative crisis, each evacuated purulent depôt, for the time, gave relief—that is, of course, by comparison with the exacerbation of symptoms ushering in its formation, and the disintegrating process, set up, perpetuated, and completed for its escape; though the pressing symptoms were ameliorated by results such as these, yet temporary indeed was the looked for consummation; the persistent discharge, the irritable sinuses, the stinking fœtor, all remained as so many sources of irritation, undermining the health, and robbing the body of all strength and vigour. Thus, until the period of his being placed under my care, matters became daily worse, both locally and constitutionally, so that life was pressed hard, closely threatened by death.

His condition, when placed under my care, was as follows:—The limb was enormously enlarged, its measurement around the joint being 17 inches, while a corresponding measurement on the sound limb was only 6 inches. From this enormous bulk, the swelling gradually tapered above and below. For 4 inches the humerus participated in the bulk, and downwards for at least 4 inches the forearm was involved. The coloration of the part was also remarkable; and on superficial or casual inspection, a great likelihood to malignant disease of the encephaloid form was manifest. The redness partook of a deep hue in many parts, relieved by patches of modena tint, while yellowish patches were interspersed freely. The several openings presented everted edges, with fungoid, spongy granulations. To the touch, there was great elasticity throughout, and pitting on some parts. When the pressure was carried deeper, matter was even forced to well up from one of the many sinuous tracks beneath. The examination in this necessary and determined way caused great suffering; and the man guardedly steadied the forearm by grasping tightly in the prone position the clammy, wasted, extended hand, with the sound one. The discharge from the several sinuses was profuse; through these sinuses, so extensive, so tortuous, and burrowing, curved probes were passed for many inches, previous to coming in contact with diseased bone; yet that the joint

was irrevocably broken up in its integrity, hopelessly destroyed in its adaptation, was sufficiently manifest from the unnatural mobility displayed in the slightest manipulation of the part. Antero-posteriorly, and laterally, the disintegration of tissues seemed equally advanced; while, from the grating within, it was clear that the articular surfaces had suffered in a like way. The evidence was confirmed by the probe, and the diseased bones tracked out with much accuracy, almost throughout their entirety. I have alluded to the hand, already so characteristic, as emaciated, sweaty, and extended: however, the patient had some power in making the muscles of the forearm act upon the fingers, and ever so slightly flex and extend them; but no power could be excited in the wrist—support being taken away, the hand drooped. All above the swelling, the upper-third of the arm and shoulder were greatly emaciated, throwing into far bolder relief the immense augmentation of bulk produced by infiltration of tissues, and new deposits, all around and about the diseased articulation. Many would suppose, after inspection of the limb—aye, and even after the careful examination of it too, that amputation was more applicable to the case than excision. So emaciated and worn had the young man become, from long suffering, that he came up to town, from the country, to be relieved from his disease. It was quite clear that his constitution could not battle on much longer; already the fatal effects of the unrelenting irritation were becoming more and more manifest. His appetite was gone for months; he was perpetually bathed in sweat, his hair falling out, his pulse double its normal frequency; repeated attacks of diarrhoea, and at times loathing, vomiting, and rejection of all food. Here, then, we have the constitutional manifestation of the local disease sapping the very life. I repeat, many would have supposed the case suitable alone for amputation. However, I did not think so, from experience in serious cases of the kind, and observing the preponderating disorganization and changes as exhibited in the soft parts, consequent upon diseased bones. I am of opinion still, as forcibly expressed in former papers bearing upon the question of excision of joints, that much weight need not be attached to this change in the superficial structures shielding-in the joint; and, as I have before written, this precept may be adduced, *Remove the sources of irritation, the dead bones, and in due time the soft parts will recover themselves.*

I decided, then, on excision in this case, having assured myself that the bones were not corrupted beyond a certain extent, and being practically cognizant of facts which could

not but bear encouragement and confidence to the true interpreter of nature's efforts in the power of compensative adaptation for parts dwindled, decayed, or lost; and thus I rested upon the hope of saving the limb. Again, the power, though a limited one, of flexion and extension of the fingers, was in abeyance; the youth of the sufferer buoyed up the hope of conservation; and, I may be pardoned for saying, the successes of dark and cheerless cases made me cling to this doctrine so forcibly inculcated in the best schools, and which I have myself so warmly espoused, and endeavoured to lift up by my feeble voice and pen, at the same time with a determined vigilance and integrity to be just, for the cause of science, and, above all, in its highest appliance, the benefit of humanity. Having decided upon operation, no time was to be lost; and, on the 22nd July, I excised the diseased bones after the following manner:—

The man being placed lying down upon the operating-table, he was steadily and quickly brought under the influence of chloroform. The affected arm was grasped by one assistant above, and thus the main artery of the limb controlled, and the shoulder steadied. The entire member was rolled inwards, so as to expose its posterior surface, and the forearm and hand steadied by a second assistant. Standing, then, on the left side of the patient, with the left hand fixing the affected joint, I plunged a strong-pointed knife into the soft parts several inches above the articulation, immediately over the posterior aspect of the limb, making a vertical incision, full six to seven inches in length, through the entire depth of the disorganized tissues, down to the bones. Blood did not flow quickly from this extensive wound; and so deep was it, that my index-finger was concealed when exploring the changes wrought within. I could feel the roughened bones freely enough now; but it was apparent that the long single incision would by no means be sufficient, or render practicable the further steps of the operation; so that the knife was carried from the vertical incision, outwards, for at least two inches, and corresponding to the line of flexion, or rather a little above the head of the radius; but, owing to the inordinate thickening of the parts, sufficient room was not yet afforded for dealing with the bones. Therefore the transverse incision was continued from the vertical, inwards, for about three inches. When the knife was first laid on, it was thrust down to the bones; but, as it was drawn inwards, a lighter hand directed it, so that the nerve should not be imperilled. The mesial and deep part of the commencement of the wound then exposed the bones, while the internal

and more superficial division permitted the more ready retraction of the flaps. The disorganized attachment of the triceps was then cut through, flexion of the forearm forced, the shreds of lateral ligaments and supporting tissues of the walls of the joints freely revealed. The end of the humerus was robbed of its cartilage, and adjacent cancellated layers, while the internal condyle and articulating trochlea for the ulna were not only deprived of cartilage, but absolutely separated from the shaft and remnant of the external condyle. The olecranon of the ulna was deeply eaten out in its sigmoid cavity, and its posterior extremity removed; its coronoid process separated and loosened. The head of the radius suffered in a similar way; while, leading from the deranged disintegrated surfaces, the shafts were mortified largely, to the removal of periosteum, and destruction of the corresponding superficial surfaces of the bones. With difficulty they were released from the matted and unnatural soft parts in which they were so deeply imbedded; this can scarcely be estimated, except by one accustomed to deal with such matters. As the knife freed the parts, being carried steadily and closely round the bones, much assistance was rendered by the occasional introduction of the thumb, index, and middle fingers, so as to surround the end of the bone attacked, and force away by pressure the soft parts from it. A small piece of linen carried in before the ends of the fingers not only assisted the manipulation by steadying the grip, but also protected them from being wounded by the sharp and detached spiculæ alluded to; each—the humerus, the ulna, and the radius—being separately dealt with, and successively, in this way, as far as the adherent periosteum and maintaining soft parts, marking out and isolating the diseased from the sound bone, as it were; at this junction the saw was applied—that saw which bears my name, and which is now so universally adopted.

The saw, I say, was employed, after the manner which I have so strongly insisted upon in similar cases; the bones being cut from before, backwards. First, section being made of the humerus, three inches and a half being taken away—the bone, when cut, presented the most healthy state,—in a like way the radius and ulna were divided—that is, from before, backwards. Ample and free exposition of their ends having been accomplished, the head and neck of the radius were removed, and the olecranon process, with the coronoid, and an inch of the shaft of the ulna. This section included all that portion of the radius above its tubercle, and the ulna at the same line. It was most pleasing to inspect their cut ends: they

were hard, healthy, natural; while the insertion of the biceps was left undisturbed—a point upon which too much practical importance cannot be placed, as preserving a salutary influence on the after-motions and perfection of the limb. Large masses of disorganized soft parts were then cleft away; but one vessel required a ligature. The limb was then placed in the box which I use for such purposes, and which will be found figured in the February Number of this Journal for 1859. The sides being let down, the limb rested evenly on its inner and ulnar surfaces. The flaps were first lifted lightly towards approximation, and retained in this state by shreds of lint dipped in cold water. The sides of the apparatus were then elevated, and the limb steadied by the protecting pads placed within. The straps were then buckled, and thus all secured.

The patient was then conveyed to bed, having quickly rallied from the effects of the chloroform; and so admirably did it act, that he was not in the least conscious of pain, and had no remembrance of suffering. When he awoke, he could scarcely be brought to believe that an operation had been performed. After being placed in bed, he got a warm draught of wine, and shortly after an opiate, to induce sleep, as he seemed more than naturally excited: evidently a state induced by a double cause, —exalted happiness, by the rescue from apprehended suffering, and the stimulant effects from the protracted inhalation of the anæsthetic. In four hours after the operation, intermediary hemorrhage set in, immediately after an attack of vomiting, so frequent a sequel after the full exhibition of chloroform. The bleeding was slow and steady at first, and by gentle pressure on the humeral artery, high up, was controlled by the resident pupil for a short time. However, whether through inefficiency, or from the blood finding a ready circuitous course on the exaltation of the heart's action, the flow came on more quickly, rapidly, and persistent, so as to create alarm. A tourniquet had then been applied, and I was sent for. It was clear that a considerable amount of blood had been lost; the bed, pads, and sheeting, bore ample marks to this effect. Several large coagula were around the wounded part. I quickly let down the sides of the box, liberated the tourniquet, and cast it off; cleared away the lint from the cut surfaces; turned out all coagula from the deep parts and fissures; let a stream of cold water trickle over all: no vessel of magnitude yielded the supply. Though warm wine was administered, and warm sponges thrust into the deep fissures, yet still the blood welled up rather profusely from several vessels—separately, with but a small stream, and of little consequence—taken



in the aggregate of moment and of great importance, essentially demanding to be checked. Having torn up several long shreds of lint, and steeped them in a saturated solution of perchloride of iron, each was passed carefully into the deepest recesses of the extensively wounded part, and so on a succession of them, until the entire was filled up. Suitable compresses were placed over these, and the limb was bandaged from the fingers upwards, with a gentle, steady support; and as the bandage arrived towards the elbow-joint, it was carried with the same moderate tightness over the compresses, so as amply to supply the place of the fingers which retained them in their position. The arm, in like manner, was lightly rolled from above, downwards; and so, due support offered in this direction, down to where the compresses lay supported by the bandage first applied. The limb adjusted in this way was again steadied in the box, its sides elevated, and the same steady position maintained. The tendency to bleeding was checked, and no appearance of blood issued from the dressings. Wine was now given abundantly, as the man was prostrated to the lowest degree. Full opiates were given every third hour, ice in the mouth to cool the tongue.

6 p.m. No return of the bleeding, but sickness of stomach very troublesome. Ordered two drops of prussic acid to each draught, and iced wine in two-ounce doses every third hour.

23rd. No return of bleeding; and though the stomach was sometimes rebellious, small quantities of fluids and ice were taken with relish. Sleep at intervals. On the whole, he is as well as possibly could be expected. Though a good deal prostrated, yet the pulse is steady, soft, and equable in its beat. The limb is at rest; no pain from it. To continue the wine iced, and draughts of opium; to have some strong chicken-broth, in small quantities, occasionally. Evening report most favourable. Stomach retains all nutriment; and he has slept quietly at short intervals throughout the day.

24th. Has had no returns of the vomiting, slept quietly, and feels refreshed. Took some toast with tea. Owing to the extreme heat of the weather, I was compelled to remove some of the dressings, pads, &c.; this was done with the least possible disturbance or shaking of the limb; the entire was sustained by several hands, so that no drooping at the site of the excision was permitted to take place. The box was then withdrawn, cleansed, and quickly replaced; caution was taken not to disturb the portions of lint thrust deep into the wounds, but all exterior to these were clipped away, and replaced by fresh portions steeped in spirit lotion. After this, I had the

man gently elevated, in the recumbent position, and placed in a fresh bed. Quickly after he exhibited all the benefits of this change, by falling into a quiet, tranquil sleep, that continued for several hours.

26th. Improved, in a most marked way, in all respects. His countenance has lost its haggard, wretched expression. He sleeps, and takes his food regularly and well. On this morning removed all the dressings from within the wound; the suppuration healthily established helped to loosen and cast them off, while a constant stream of tepid water completed their separation without the least pulling or violence, a point to be laid the greatest stress upon, as a repetition of the bleeding, at this stage of the case, might easily compromise the favourable issue. On the 28th, commenced to give gentle support, and draw up the flaps towards each other with adhesive straps and bandages, all the time cautiously guarding against either shaking of the limb or changing it from its flexed position. Every alternate day this treatment was adopted, and so likewise changing the patient's bed linen, &c., &c. Quina mixture, with small quantities of acid, administered to check some tendency to sweating. Opiates now only given at night. Strong broth, eggs, wine, chop, porter, for nutriment. Under this management, the case progressed most favourably, up to August 8th, when his state is reported in these words:—His emaciation is already greatly removed; he sleeps quietly through the night, without disturbance, and sweating has ceased; his appetite has much improved, and he enjoys his food; his whole countenance has undergone a marked change; he looks intelligent, cheerful, and happy, and speaks with a certainty of soon being well; his pulse is full, steady, and in number 70; the bowels and kidneys perform their functions healthily; and altogether, the constitutional manifestation of relief and comfort is an excellent harbinger of recovery. On this date, the condition of the limb evidences a marvellous improvement, gradual and steady up to the present time—the precursor of that favourable state just described, and stamped on the countenance of the man. The diminution of the bulk of the limb is almost wonderful; in fact, it is one of those changes that must be seen to be believed. The engorged, swollen, puffed-out, unnatural-looking soft parts have altogether put on a new aspect; the sinuses are nearly all obliterated, but little matter weeps from them; the skin, as it were, has regained its elasticity, and disgorged, forced out, the discoloured serum from its tissues; all the deep incisions are consolidated and bound together within; no matter wells up on

compression of the parts; the more superficial portions of the extensive wounds are healed by growth of surface into surface, and the new skin is rapidly creeping in from the margins around, and covering the healthy granulating parts; no more matter is secreted than that sufficient for the protection and well-being of the process of cicatrization; no pain is complained of during the dressing of the part; and the same gentleness, steadiness, and support is as gradually afforded as in all the earlier management of the case; the forearm is kept steady, at a right angle with the arm, due care being taken that the adhesive straps and bandages shall insure their relationship, as well as bind the bones closely together, and thus limit the extent of uniting medium; the box sedulously enforces and guards these several requirements.

August 19th. Up, and walking in the garden. Limb doing well.

December 17th. All nearly healed, and full power in bending the fingers and wrist. The power of flexing forearm, of course, not yet regained.

February 1st. He went to the country, with the limb quite healed, and general health restored.

March 2nd. Had a most favourable account from him. All the motions of the arm steadily returning. He is able to grasp and lift weights with it, and make it useful in all its under motions, while, in a limited way, he is regaining the upward movements. I have beside me this moment a letter which I only received a few days ago from him, and in which he describes his condition as that of perfect recovery; all the motions of the limb being conducted with a steadiness and precision but little different from the sound arm; it is not yet so strong as the right one, but sufficiently so for all ordinary purposes. The conducting of this case to so happy an issue, I look upon as a great triumph to Conservative Surgery. Certainly everything seemed dark and gloomy about it, so closely was he run down, when he came under my charge—the preservation of life, even by dismemberment by amputation, being a proposition that might stagger the most sanguine of success.

I shall conclude this paper by detailing a case in which I excised the entire radius from one articular surface to the other.

**CASE IV.**—*Excision of the radius from one articular surface to the other; recovery, with almost perfect motions and functions of the limb preserved.*

Joseph Falkner, aged 17 years, admitted to Mercer's Hospital, January 1, 1859. Three weeks before this date, the boy met with the following accident, produced in a very unusual way:— he was rapidly running down stairs, in the dusk of the evening, his right hand resting upon the bannister as he slid along; suddenly his right heel was checked by one of the stairs, and he would have been precipitated to the bottom, but for the power with which he grasped the bannister; and so he swung violently round, with so much force as to strike his legs against the rails, and his arm, too; he suddenly let go his grip, and fell down, suffering the most severe pain in the wrist. His mother stuped him, &c., and managed him after her own way, the boy at the same time being very unruly, and incapable of being restrained from going to play with his companions; latterly matters assumed a more serious form: the limb became more painful, red, and swollen, and the boy finally could not obtain any sleep. Thus he was brought to hospital, haggard, thinned, worn out from want of rest and inability to take nourishment; the hand was swollen; the forearm red and discoloured, and enlarged by œdema and engorgement, highly sensitive to the slightest touch. On examining the limb, I readily detected the solution of continuity; the epiphysis was wrenched off from the lower end of the shaft of the radius, while the bone was again smashed high up below its tubercle, and the insertion of the biceps; thus the solid bone suffered, while conjointly there was such laceration of soft parts, stretching and tearing of ligaments, that inflammation had run its course, and extensive abscesses passed up the entire length of the forearm, on the posterior surface, and even between the bones. The patient was so wretched, stricken by starvation and loss of sleep, that I confined him to bed, communicated soothing heat, coaxed him by palatable food to eat, compelled sleep to settle on him, by narcotics, for some hours. I then cut freely down upon the matter, and allowed it to escape, slit the tightened fascia so as to relieve constriction and congestion. I next bandaged the limb from above downwards, and from below upwards, leaving the wound free for escape of all secreted and exuded fluids. The fore-arm and hand were then steadied upon a splint, so as to secure against the least disturbance of the disrupted parts: most liberal diet, quinia.

January 8th.—The lower part of the limb is much im-

proved; constitutional disturbance subdued. He sleeps the entire night, and eats well; another abscess forming higher upon the back of the limb, close to the olecranon process; this is on the seat of fracture; this likewise had to be opened, so as to guard against thumbing, pressing, and squeezing the matter towards the lower opening; the entire limb was then carefully bandaged, and steadied upon the splint.

11th. There is a connection established between the upper abscess and the cyst of the lower, so that the discharge of the two nearly all flows off by the most dependent or lowest aperture.

13th. Had to make an opening on the forepart of the limb, about its centre, owing to matter burrowing in this direction; supported the entire by pads, bandages, and splints; wine, Dover's powder, bark, beef-tea, and wine, freely.

19th. Pressure carefully made each alternate day from above down, and from below upwards, with bandages, the entire steadied on the splint. Thus matters went on gradually mending, up to March 10th, when I had freely to lay open the parts over the external condyle, and give exit to matter, after gentle support; wine, quina, &c.

On March 17th, the limb assumed a more serious aspect, marked indications of increased bulk and swelling, great pain, and the supervention of a brisk attack of erysipelas; this again was assuaged, cured by suitable treatment.

On the 23rd, had to open another extensive abscess above the internal condyle.

On the 30th, dismissed to attend as an extern, so diminished was the discharge, in the aggregate, from all quarters; many of the abscesses were entirely healed; and, though I had my misgivings about the condition, the integrity of the bone, yet no sufferings created alarm; on the contrary, his general condition was visibly mended, and his strength materially built up. He was removed by his friends at this time to the country. However, on the 2nd of December he was re-admitted, with profuse discharge from the limb, streaming from several openings, characteristic of the presence of diseased bone. On examination with the probe, I detected the radius at its lower part denuded, rough, and gritty; while, again, on its central part it was stripped of its periosteum, hard, and resonant. Higher up again, close to its tubercle, it seemed deprived of vitality; a good deal of dense thickened material was thrown into the soft parts around, and so filled the inter-osseous space, as to remove altogether the symmetry of the forearm. Again, this compensating material, this lymph effusion, was laid down in far

greater proportion on the posterior aspect of the limb. When he was brought back to hospital, it was stated, that, after being at home for some time, and the limb nearly recovered, he was thrown from a car, and the delicate arm again severely bruised and crushed; and though kept quiet, and the part constantly stuped, the swelling continued to increase, and soon broke, a quantity of matter being discharged by an opening close to the wrist, and in the site of that which had previously existed there, but until then had remained healed from the time of his leaving hospital. A second opening quickly followed in the centre of the bone; and the original one, high up, never healed, and now discharged more freely than ever. On careful examination, it was quite clear that the radius had perished throughout its entire extent, with the exception of its articular surfaces and neck; and I determined on at once excising the bone, to relieve the constitutional symptoms, so seriously prostrating the child, and threatening life.

On the 3rd of December, the boy was brought into the operating theatre, placed lying down, and brought under the influence of chloroform. I selected the posterior surface of the limb as the direction in which the incision should be made for the extraction of the dead bone. A long incision was carried upon the posterior surface of the thickened covering of the two lower thirds of the radius. The edges of the soft parts being drawn aside with copper spatulæ, and also to a slight extent detached or lifted away, a strong scalpel was thrust through the more dense material, and completed throughout this trajet, perfected section down to the diseased bone. With some additional force, this elastic consolidated material was made to recede, so as to allow a more accurate examination. The radius lay in front of this reparative material, it was ascertained as before detected, separated from its epiphysis below, it was rigidly, however, incased all along, and perfectly immovable above. I then made a second incision over the neck and tubercle of the bone, about two inches in extent; and after meeting with some resistance from the same characterized material as already alluded to, I introduced the blades of a long fine bone-forceps, and clipped across the radius below its tubercle. Having done so effectively, I next, with a very strong forceps, grasped the shaft of the bone, and, after some twisting and forcible traction, extracted the entire radius through the extensive wound below. This was most satisfactory, as there remained after, a smooth hollow recess only, corresponding to the posterior three-fourths of the bone. No new material protected it in front; and in the lower part or third of

the arm, the pulsation of the radial artery was quick, distinctly felt from behind. The only protecting barrier seemed to be a slight thickening or condensation of the intervening tissues in the normal relationship between the vessel and the bone. The exterior incision below was not unattended with hemorrhage; on the contrary, it was sharp and rapid—two considerable vessels had to be ligatured. By the shorter incision above promoting easy section of the bone, without subjecting the part to additional violence, or the system to loss of blood, I conceive, a practical fact is arrived at—a good lesson is taught. A few small vessels required to be ligatured, and then the vacant bed left by the extraction of the radius was filled by a few long shreds of lint, soaked in oil, a bandage applied, the limb steadied on a splint, and the boy removed to bed. After a few days, the lint was removed; quickly granulations made their appearance, of a granular, healthy character; soon the hollow was filled up, quickly the edges of the long wound were approximated, drawn together, united; and, in about three weeks after the operation, the limb was healed throughout. It was solid, firm; and, at the end of six weeks, he was able to close the fingers perfectly, and to extend them, to flex the arm and extend it as well as ever, pronation and supination of the forearm being quite deficient. The new material seemed matted to the ulna; but it was a sufficient bond between the lower and upper ends of the bone—a sufficient sustentation for all the required movements, either delicate and refined, or coarse and violent, save two, pronation and supination,—easily compensated for, however, by neighbouring joints.

The whole case, in its management from beginning to end, teaches a valuable lesson: how youth will repair accidents of the gravest nature, by the vitality of its tissues; how that, even with the severest complications, results may be expected that never could be anticipated in later years; how that when, through additional injury, from accidental circumstances, all nature's efforts at repair may be broken up and destroyed, and absolutely a large portion of the solid part of the limb perish by death, yet how that, in youth, this deadened bone, fatal to life, if permitted to remain, may, by a bold and dexterous operation, be removed; and how it may be replaced by a substitute; and how the limb may be restored to nearly the full performance of all its functions,—*by Conservative Surgery*.\*

\* The casts taken before and after operation in these several cases, as well as the diseased parts and bones cut out, are carefully preserved in my Museum.

ART II.—On Croup. By E. WHITTLE, M. D., M. R. I. A.,  
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IN the present day there appears to be, amongst medical men, a degree of uncertainty as to the best mode of treatment which can be adopted in croup. I believe this difference of opinion arises, in some measure, from the confounding of different forms of laryngeal affections,—forms which, when accurately distinguished, will be found to require, in some respects, a very different line of treatment.

I purpose, in this paper, suggesting some points of distinction in the various kinds of croup that are commonly met with in practice, and then detailing the mode of treatment which I have found most applicable to each form.

For practical purposes, I would divide croup into seven varieties: strictly speaking, these cannot all be regarded as real croup; but they are all affections of the larynx, attended with constriction of the glottis, and all liable to be confounded together under the name of croup.

1. *The Cynanche Trachealis* of Cullen: true croup, with formation of false membrane.

2. *The Angina Stridula* of Bretonneau: pseudo-croup of Guersant; acute asthma of Millar.

This is a form of croup characterized by spasm of the glottis, with intense inflammation of the lining membrane of the larynx and trachea, but without formation of false membrane. This form of croup has not been generally distinguished in the child from true croup. Bretonneau and Guersant endeavour to distinguish them; but, throughout the whole of their observations on cases of this kind, they create confusion by confounding this form, which is in fact an acute laryngitis, with laryngismus stridulus, which is purely a nervous affection.

3. *Croup, complicating Diphtheria*, the diphtheritic exudation gradually extending into the larynx.

4. *Sympathetic Croup*, connected with the exanthematous diseases.

5. *Spasmodic Croup*, excited by an ulcerated state of the larynx.

6. *Mechanical Croup*, caused by œdema of the glottis.

7. *Nervous Croup*, Laryngismus stridulus.

1. *Cynanche Trachealis*.—This form of croup is characterized by the suddenness of the attack, and by its generally coming on in the night. A child goes to bed apparently quite well, and after two or three hours' sleep awakes suddenly, nearly



suffocated: the symptoms are so urgent, that, when remedies are not promptly applied, death frequently takes place in a few hours. The symptoms of this form of croup are so familiar to us all, that I would not allude to them, except for the purpose of noting the points of difference between this and the second form.

2. *Cynanche* or *Angina Stridula*.—Croup, with spasm of the glottis, excited by the inflamed condition of the mucous membrane of the larynx and trachea, but without formation of false membrane. This is, in Liverpool, the most common, the most dangerous, and the most fatal form of croup; but is, I believe, almost universally, in practice, confounded with *cynanche trachealis*. It commences with an occasional hoarse cough, which generally excites very little attention on the part of the parents or nurses: this sometimes lasts for two, three, or four days; perhaps a little cough medicine may be given, which affords some temporary relief; but at last the breathing becomes croupy, and then, for the first time, the parents take alarm. When the doctor arrives he will find the child in a high fever, with breathing very quick and hoarse, but not so loud as in true croup; and every now and then the child coughs with a sharp, ringing, barking sound, which is quite characteristic of this form of croup. The peculiar noise of the cough seems to be occasioned by the forced expiration of a volume of air through the contracted glottis, by each act of coughing. If a child in this state is treated in the old routine way, by leeching, emetics, antimony, and calomel, a little temporary relief may be afforded; but, at the end of a few hours, the breathing becomes gradually more stridulous, the child throws back its head, and struggles almost convulsively for breath; throws its arms about, and stretches forth its hands, as if imploring assistance; the pulse becomes very weak and rapid, and copious perspirations stream down the child's neck and breast. If tracheotomy be not now performed, the child very quickly dies, suffocated.

This form of croup is accurately, though only incidentally, described by Bretonneau; but he appears to consider it of little importance. I quote from his second memoir on diphtheria, as republished by the New Sydenham Society: in speaking of the treatment of diphtheria, he says, "a simple tracheitis, or even a very mild sporadic affection, which is, perhaps, the same as that designated by Millar his acute asthma in the first stage, has made the reputation of the most accredited methods. This sporadic affection, which simulates croup, is not at all rare; it commences as it terminates, that is to say, from the

beginning, the alteration of the tone of the voice, the peculiar sound of the cough (resembling the barking of a young dog, heard at a distance), and the spasmodic difficulty of respiration, would cause the most fearful apprehensions, if any fears were not counteracted by the rhythm of the circulation, which is not so much disturbed as it is accustomed to be at this stage of true croup, and if we had not an additional reason for security in the natural state of the pharynx, and in the absence of swelling of the lymphatic glands, which in epidemic croup are constantly tumefied in the regions corresponding to the affected membranes."

Possibly in the comparatively dry climate of central France, this form of croup may not be serious. A sub-acute laryngitis may take place in a child and subside in a few days, without giving rise to alarming symptoms; but I unhesitatingly affirm that, in our cold and damp climate, particularly in the winter months, this affection is the forerunner of the most fatal form of croup: possibly, if life lasted long enough, false membrane might form; but I have always found that death ensues a very few hours after the commencement of the spasmodic breathing,—indeed, I do not think the child ever survives more than from twelve to sixteen hours, though the incipient stage, described by Bretonneau, may have existed for several days, or even for a week.

At page 115 of the same work, we find a chapter devoted to the diagnosis of this form of croup, as distinguished from croup caused by diphtheria; but, throughout the whole chapter, he appears to confound it with laryngismus stridulus (a purely nervous affection). But still, in describing the severest case that he had met with, he concludes with the remark that "the mucous membrane of the larynx was undoubtedly affected with a rather acute catarrhal inflammation." Under the name of "tracheitis," he then describes three cases of this disease, two of them terminating fatally. The post-mortem appearances are thus described:—"The base of the tongue was very red; the mucous membrane of the trachea and bronchi was thickened, and of a uniform deep red colour; the back of the mouth was filled with mucous matters of a grey-greenish tint." The state of the larynx is not specially mentioned.

The second case recovered.

"CASE III.—The opening of the body confirmed the opinion which had been offered, that no false membrane existed in the trachea; the mucous coat of the larynx and trachea was thickened, and of a very bright red. A small quantity of

creamy puriform mucus (about three drachms) filled the first divisions of the bronchi. I did not pay sufficient attention to ascertain whether the dimensions of the glottis were sensibly diminished by the tumefaction of the mucous folds of the larynx. It did not appear possible that the mucus accumulated in the principal divisions of the bronchi would have opposed an insurmountable obstacle to respiration."

I have made two post-mortems in cases analogous to these described by Bretonneau; in one the post-mortem appearances were exactly the same; in the other, besides the redness of the mucous membrane, the glottis was contracted by chronic thickening of the folds of the mucous membrane and sub-mucous interstitial deposit; but in this case the child had been, for more than two years, subject to repeated attacks of croup, which eventually became chronic, an acute attack supervening whenever the child caught cold. It was eventually carried off in a few hours by one of these attacks, the parents refusing to consent to the performance of tracheotomy. Guersant distinguishes this form of croup from the membranous form, but, like Bretonneau, writes as if he did not look upon it as at all a formidable ailment. Jurine appears to have described it as intermittent croup. Of three cases which he describes, one would appear to have been fatal, and the post-mortem appearances seem to have corresponded with those described above.

Bouchut distinctly confounds this disease with laryngismus stridulus. At page 279 of the work of the New Sydenham Society, quoted above, we read:—"The disease which has been most frequently confounded with croup, and which has given rise to mistakes which are fatal in their results, is false croup, or stridulous laryngitis. It is important to establish a precise distinction between these two affections, which require entirely different therapeutical appliances. M. Guersant has contributed more than any one to clear up this subject, which remained so long confused and obscure."

"Stridulous laryngitis is accompanied, like croup, with a dry, hoarse, sibilous, and more or less sonorous cough. The difficulty of breathing is extreme,—the child appears as if about to perish of suffocation; still the larynx is free, and there cannot be any expectoration of false membranes. The phenomena which are observed are purely nervous; they soon subside, and their progress is altogether peculiar. They appear suddenly, and in a very high degree of intensity, in subjects who are otherwise in good health, or slightly affected with cold. They manifest themselves in the middle of the night;

the paroxysm lasts about two hours, and is reproduced on the following two or three nights in succession; but it becomes gradually more feeble, and at last it disappears."

Here are stridulous laryngitis and laryngismus stridulus, two distinct and formidable diseases, completely confounded together, and both treated as trivial complaints; while one is a most dangerous form of sub-acute inflammation, and the other, though simply a lesion of innervation, yet not infrequently fatal in its effects.

To resume, this form of croup is distinguished from cynanche trachealis, by the occasional barking cough existing for two or three days previous to the acute symptoms setting in; the same feature, with the sthenic character of the accompanying fever, and the absence of white patches in the throat, will distinguish it from croup complicated with diphtheria. How it could be confounded by a practical physician with true laryngismus stridulus, I can scarcely conceive,—the latter being a purely nervous affection, generally, if not always, arising from the irritation of dentition, always coming on suddenly with a slight convulsion, followed immediately by a loud, crowing, inspiration; these symptoms sometimes recur again and again, with brief intervals; but, throughout the attack, the breathing is not at all croupy,—the crowing sound of the inspiration being the only resemblance to croup that presents itself.

3. *Croup complicated with Diphtheria.*—This is really diphtheria, gradually extending into the larynx. Before the symptoms of croup present themselves, the condition of the tonsils will have sufficiently declared the true character of the attack.

4. *Symptomatic Croup.*—This is a rare form of croup, occasionally met with in the early stages of the exanthematous diseases: it sets in suddenly, with high fever, and very loud croupy breathing; if the eruption comes out well, the croupy symptoms will, I believe, subside spontaneously. I am not aware that there is any symptom by which we can recognize a case of this kind, until it is declared by the appearance of the characteristic eruption. I have myself met with but one case of this kind. The patient was a strong working lad, fourteen years of age; I found him in a high fever,—skin dry, pulse sharp and quick; breathing very loud and croupy. I bled him freely from the arm: this mitigated, but did not entirely remove, the symptoms. At the end of a few hours the rash of rubeola broke out all over him, on which the croupy symptoms entirely disappeared, I must beg the reader not to con-

found this form of croup, which is very rare, with those cases, pretty often met with, in which different forms of croup arise, in the advanced stages of rubeola, scarlatina, variola, and erysipelas.

5. *Croup caused by an ulcerated Condition of the Larynx, either Syphilitic, or following the Ulcerated Throat of Scarlatina or of Variola.*—I believe that in this form the immediate cause of the croupy breathing is spasm, excited by the irritation of the ulcers.

6. *Mechanical Croup.*—Œdema of the larynx, generally a complication of œdematous erysipelas, or ulcerated larynx, may, after being long chronic, give rise to mechanical croup, by causing a puckered condition of the glottis; in either case, nothing can give relief but tracheotomy.

7. *Nervous Croup: Laryngismus Stridulus.*—This is purely a nervous affection, generally a complication of dentition; it appears to be very much connected with the constitution, and in some children is excited by various causes,—over-feeding, cold, &c., but generally when the child is teething. It comes on suddenly, with a convulsion, generally slight, but sometimes of a more severe character; this convulsion is followed by a shrill gasping inspiration, something like the whoop after a paroxysm of hooping-cough, but never so long nor so loud; it is often cut short by a slight return of the convulsions, and so repeated again and again for several minutes. The attack then passes off; but, in children who are very subject to it, is often repeated once or twice in the same night, with intervals of one or two hours. It is only occasionally that medical men have the opportunity of witnessing these attacks, because, by the time a medical man arrives, the child has quite recovered; but it sometimes happens that the child is suffocated at the very outset of the attack. In these instances death is always sudden; the child may be as well as usual, perhaps sitting on its mother's lap, when it suddenly becomes stiff, struggles for an instant, and dies before the crowing inspiration takes place at all. I believe that many children who are said to die of fits, or to be overlain, die of this affection. I have myself known two children to die exactly as I have described these fatal cases; and these two children were both very subject to laryngismus stridulus. In each case I was sent for immediately, arrived in a few minutes, and yet on each occasion the child was dead: one was nine months, and the other sixteen months old.

With regard to the pathology of this disease, I have always observed that it is most met with in families of a strumous

diathesis; and I believe it is a nervous lesion, excited by different irritating causes in weak strumous constitutions; and this, I am inclined to believe, is the true explanation of Dr. Ley's theory, that it is occasioned by the pressure of enlarged cervical glands on branches of the par vagum. If his theory were the correct one, why should the affection be so irregular in its periods of attack, while the glands continue enlarged? And how comes it that we often see children with masses of enlarged glands in the neck who do not suffer from this affection; while, in very many instances, in which children are very subject to it, there is hardly any evident enlargement of the glands at all?

*Treatment.*—Of late years I have, in the treatment of croup, widely departed from the old orthodox plan of bleeding, and administering tartar emetic and calomel freely: I do not claim any originality in this, because I am aware that a feeling has been for some time gaining on the profession, that the treatment of croup had long been of too severe and energetic a character; but that very many practitioners still have faith in the old system is, I think, evidenced by the fact, that the New Sydenham Society have re-published Bretonneau's recommendation of what I think may, without impropriety, be termed the immoderate use of calomel, without a single note of objection; neither, in any critical notice of the work, have I seen any exception taken to this treatment. Since I ceased to bleed and give tartar emetic and calomel, I have been much more successful in the treatment of croup. For *cynanche trachealis* I have recourse, in the first instance, to the warm bath and an emetic: this will arrest an attack in the early stage as effectually, if not more effectually than leeching. If the symptoms persist, I administer small doses of compound ipecacuanha powder, with nitrate of potash, every hour, and apply hot fomentations to the throat; this treatment, without lowering the strength of the patient, will do more to abate the croupy breathing than calomel or tartar emetic. If the tongue is foul, or the bowels loaded, I give a dose or two of calomel with the first doses of Dover's powder. If, in spite of treatment, the disease progresses I would perform tracheotomy early, before the patient's strength was too much reduced, and before the false membrane became deposited far down the trachea: I say I would, because, though I have often recommended the operation in such cases, I have never yet been able to get the consent of the friends until it has been too late to perform the operation with the slightest chance of success.

For *stridulous angina*, as I have described it above, I would pursue the same course; but in this form of croup the point is

to attend to it before the urgent symptoms come on, during the premonitory stage, which may last for several days: even then it will take from four to five, or even seven days, before the child can be considered safe; that is, before the shrill, barking cough has completely disappeared. If no attention is paid to the child until his breathing becomes decidedly croupy, medical treatment is of little avail; and, if not affording relief in a very few hours, should be abandoned, and recourse had to tracheotomy: and it is in this form of croup that tracheotomy is pre-eminently successful, because there is no false membrane to stop up the tube, or produce suffocation by lodging below it.

Last January, I saw, one night, a little child, two years old, who had a barking cough for two or three days; his breathing was croupy when I saw him, but the case did not appear very urgent, so that I hoped it was not too late to relieve the symptoms by medical treatment. I saw him early in the morning, and found that the case had then become much worse, in fact, that suffocation was imminent: I recommended tracheotomy to be performed immediately. The parents consented, and the operation was soon performed. Just as I was opening the trachea, the child gave a sudden struggle, the trachea slipped off the hook, the air rushed through the opening, and the wound was immediately filled with bloody froth, and the child fell back, apparently lifeless; I immediately passed a probe into the trachea, raised the child's head, and then, after he was relieved by a convulsive cough, introduced a small forceps, by which means I held the trachea open for a little until the child breathed freely, and then I succeeded in getting in the tube; as soon as it was adjusted, the child was quite at ease; he steadily improved, and in a few days was able to walk about his room. Whenever the tube was withdrawn, he began to breathe rather in a croupy way; on this account, I allowed him to continue the use of the tube for some months. It was not finally removed until May; the orifice closed in a few hours; he has enjoyed perfectly good health ever since.

The treatment of *diphtheritic croup* is rather foreign to the object of this paper, so I will merely observe that I believe, when diphtheria becomes complicated with croup, the best chance for the patient would be an early recourse to tracheotomy. For *sympathetic croup* we must necessarily pursue the same treatment as for *cynanche trachealis*; from which it can only be distinguished by the appearance of the rash, upon which making its appearance, the croup will spontaneously subside.

*Croup caused by ulcerated larynx* may be often treated successfully by tracheotomy. I have three times performed this operation after scarlatina. In the first case the child did well for a fortnight, though he was anasarcaous at the time of the operation, but at the end of that time he was carried off by an attack of acute bronchitis; in these cond case the operation was unavailing, as the child died the next day; in the third the case went on very favourably for several days, but the child was very badly nursed, and no care taken to keep the tube clear of mucus, consequently the improvement did not continue. All these cases were operated on under very unfavourable circumstances. While this form of croup is chronic, the treatment will principally consist in the application, topically, of solution of nitrate of silver, or such agents, to the larynx itself.

*Mechanical croup* from œdema of the glottis, if severe, can only be relieved by a timely operation; the hindrance to breathing being purely mechanical, the relief given in these cases is instantaneous, but the operation should not be too long delayed.

With regard to *laryngismus stridulus*, the treatment is so opposite to that indicated in true croup, or stridulous angina, that it will be sufficient merely to allude to it as consisting, at the time of the paroxysm, in dashing cold water on the child's face, or plunging its feet into hot water, or in fact doing anything which will give a sudden shock to the system, and, in the intervals, strengthening the child by the use of tonics, and by having it almost constantly in the open air.

In conclusion, I must again express my surprise that nearly all the French authors, whose writings have been translated by the new Sydenham Society, seem to concur in misunderstanding this nervous or false croup. The passages I have quoted above from Bretonneau, Guersant, and Bouchut, show that they have all confounded stridulous laryngitis with nervous croup, at the same time that they seem to think more lightly than we do in this country of the danger of either complaint. Even in the brief memoir of Daviot, he seems, in the following passage, to fall into the same error. Writing on the *sonitus crepitans* of Albers, he observes: "Now to apply to pseudo-croup, which requires only the treatment applicable to slight colds, the medication necessary for tracheal diphtherite, would be to employ therapeutical measures which are not only useless, but are even likely in themselves to prolong the duration of the disease."

The term pseudo-croup will be understood in England to



apply to laryngismus stridulus, while his allusion to the treatment applicable to a slight cold shows that, while he wrote, he was thinking of the premonitory stage of stridulous angina.

On referring to Dr. Copland's Medical Dictionary, I observe that the same mistake of confounding these two forms of croup prevails throughout the whole of his elaborate article on croup.

This paper would be hardly complete without notice of yet one other form of croupy breathing, caused by the pressure of tumours, generally aneurisms, on the recurrent nerve. It is difficult to suggest a remedy for this form; the symptoms are generally remittent; but I can imagine cases in which tracheotomy might be justifiable, as a means of affording temporary relief.

P. S.—The following case, being of some interest in respect of the question of practising tracheotomy in croup, I am induced to add it to the above paper as a postscript.

November 6th. I was this evening requested to see a child three and a half years old, which then presented the appearances that I have enumerated as characterizing the early stage of stridulous angina. The child was playing about the house as usual, and now and then coughing in the peculiar shrill barking manner that I have described. I warned the parents of the child's danger, and ordered a warm bath, and a diaphoretic mixture. The next day I found the child worse. It had not had the warm bath, and had even been allowed to go in and out of the house. The breathing was now quite croupy, the barking cough more frequent, and the child labouring under a good deal of fever. This day the child was more carefully attended to, and in the evening there was a decided improvement, though the symptoms of croup were still present, but more mitigated in character. On the morning of the 8th the improvement was less marked; and in the course of the day it became evident that the child was gradually getting into a hopeless condition, as far as the administration of medicine was concerned.

Under these circumstances, and being convinced that no false membrane existed to interfere with the subsequent treatment, I recommended tracheotomy being had recourse to: this was strongly objected to by the friends, but eventually they consented, after I had long urged the necessity of the operation with a good deal of decision. The operation was accordingly performed, about 10 o'clock on the night of the 8th. The child's breathing immediately became natural; she slept

two or three hours that night. In three days she was running about the room; and on the 27th, nineteen days after the operation, I finally removed the canula; the child has continued quite well ever since. A little muco-purulent phlegm was expectorated from time to time, through the canula, but no false membrane whatever—this result of the operation confirming my diagnosis, that this was not a case of cynanche trachealis. Still, so urgent were the symptoms, that, but for the operation, the child must have sunk in a few hours, probably in five or six.

**ART. III.**—*Pseudo-colloid Ovarian Tumour, weighing 28 lbs., in which the operation of Ovariectomy was performed successfully; with Remarks.* By D. LLOYD ROBERTS, M. D., M. R. C. S. L., and L. S. A.; on the Medical Staff of Saint Mary's Hospital for the Diseases of Women and Children, Manchester; Fellow and Honorary Local Secretary of the Obstetrical, and Corresponding member of the Epidemiological Societies of London, &c.

ON August 25, 1860, I saw, for the first time, Rachel B. of Lombard-street, aged 35, married, and having three children.

*History.*—Menstruation commenced at 14; generally lasting seven days, and has been very copious for the last twelve months. It is also, in general, "difficult" for the first two days of each period. Yellow leucorrhœa has prevailed during the intervals of each monthly crisis, both before and since her marriage. She has been more or less under the care of a medical practitioner during the past twelve months, who supposed her case to be pregnancy. She first noticed "a swelling" in the right side of the abdomen, fourteen months ago. The menstrual flux still continuing, and the abdominal tumour also enlarging, and not feeling satisfied with the prolonged duration of the tumour, as the full period of utero-gestation had passed by, she determined upon seeking other advice, and accordingly I was requested to see her. On my visit, I found the patient a tall, spare woman, of sanguineous temperament, and looking much emaciated, though (she informed me) she had previously been stout and healthy. On making an abdominal examination, I ascertained that that cavity was enormously distended by ascitic fluid; and, on making firm pressure to the right of the umbilicus, a solid resisting tumour could be distinctly felt. In addition to the emaciated condition before mentioned, there were an anxious expression of the coun-

tenance, a small, weak, and frequent pulse, anorexia, the foul tongue of sulphuretted hydrogen dyspepsia, constipated bowels, sleeplessness, dysuria, and frequent desire to void urine: the latter, after standing a short time, presenting a copious deposit of lithates. I prescribed a sedative draught, to be taken at bed-time, and, after explaining to the patient the nature of her case, informed her that I should call upon her next day to tap her. This I accordingly did, and was accompanied by my esteemed friend, Mr. Runcorn, of St. Mary's Hospital, who at once agreed with me as to the nature of the disease. She was then tapped, and 14 pints of thin and highly albuminous ascitic fluid were drawn off. The abdomen being then examined, a large solid tumour was found, occupying each side of the abdominal cavity, on a level with the umbilicus, extending upwards to midway between the umbilicus and the scrobiculus cordis, and downwards, on the right side, into the iliac fossa. It could easily be rolled from side to side; and the patient, when turning herself in bed, instinctively supported the tumour with her hands. A vaginal examination disclosed that the uterus was somewhat prolapsed—a condition arising probably from the pressure of the tumour above; it was also perfectly movable. Elevation of the tumour by pressure with the hands was found to produce but slight alteration in the position of the uterus—thus indicating that the tumour was not connected with that organ; and, on examining with the finger on all sides of the body of the uterus, as far as the walls of the vagina would allow, the uterus and the tumour appeared to be entirely unconnected. A pad was now firmly applied to the abdomen, by means of a broad binder; and half a grain of hydrochlorate of morphia was administered in a sedative draught. We then left. In fifteen days the peritoneal cavity had become again very much distended, and tapping was again resorted to. It was performed this time by Mr. Runcorn, and the quantity of fluid which was drawn off by the canula, together with that which oozed away afterwards, was estimated at 13 or 14 pints. She was now recommended to become an in-patient of St. Mary's Hospital, into which institution she was admitted on September 27. Two days afterwards a consultation on the case was held, and ovariectomy decided upon, as affording the only chance of preserving the life of the patient. She was also seen by my friend Dr. Radford, who concurred in the opinion of the desirability of ovariectomy. I made the result of the consultation known to my patient, and fairly laid before her the formidable nature and danger of the operation. She bore the communication with the fortitude of

a Christian, and consented, "if I thought there was no other chance," to place herself entirely in my hands. The few succeeding weeks were occupied in preparing the patient for the operation; during which time the ascites so much increased, that I deemed another tapping advisable. This was performed on October 19, when 9 pints of fluid were drawn off, and a large quantity (estimated at 10 pints) oozed away in the course of the three following days. The treatment, from her admission to the time of the operation, consisted principally in supporting the system by good diet, and in giving her iron and quinia, diuretics, iodide of potassium, &c.; and the following sedative pill to relieve pain:—Hydrochlorate of morphia,  $\frac{1}{2}$  grain; extract of cannabis Indica, 1 grain; extract of hyoscyamus, 2 grains.

On November 1, I performed the operation of ovariectomy, in the presence of my colleagues and a number of professional friends; chloroform having been carefully administered by Mr. Runcorn.

Considerable difficulty was experienced in bringing the patient fully under the influence of the anæsthetic; and some vomiting took place, notwithstanding that (in anticipation of the chloroform) all food, with the exception of one cup of tea and a little dry toast, had been purposely withheld from the patient that morning.

The operation was commenced by making an incision three or four inches long, from one inch below the umbilicus downwards. The peritoneum was in this manner exposed, and then divided on Key's Director: some ascitic fluid now escaped, but not much, as she had been tapped only fourteen days before. The peritoneum covering the tumour was next carefully divided on the director, and a large cyst came into view. This was opened by Mr. Spencer Wells' large trochar, and about 10 pints of thick mucilaginous fluid were rapidly drawn off. It was now perceived that the remaining, and by far the larger portion of the tumour was solid, only a few small cysts being scattered here and there on its surface. I passed my hand carefully round the tumour, and found that there were no adhesions.

My friends, Dr. Radford, Dr. Stephens, and Mr. Lynch, the two latter of whom were kindly officiating as my assistants, here suggested that the incision should be enlarged, as it seemed altogether insufficient for the passage of the tumour; accordingly, it was extended above and below, to the length of nine inches. Through this opening I drew the tumour,

which was then firmly held by Dr. Stephens and Mr. Lynch. The pedicle, about four inches long, and an inch and a half broad, was tied by being transfixed twice by strong ligatures, and was then divided close to the tumour. The surface of the peritoneal cavity was now carefully cleared, by means of a sponge, of some fluid and clotted blood which adhered to it; the left ovary and the uterus were next examined, and found healthy; and the wound was carefully closed by means of four silver hare-lip pins, passed through the divided edges of the abdominal parietes, including the peritoneum, and the adaptation of the edges still further secured by superficial sutures of silver, which were placed in the intervals between the pins. The pedicle was brought outside, and secured by passing a superficial suture through it and the integuments, to prevent its receding into the abdominal cavity. Strips of plaster were used to steady the ligature, and the skin was also protected from the edges of the pins, in the same manner. Strips of plaster extended across the abdomen, and the application of a pad of cotton-wool and a flannel binder, completed the dressing. The patient was put to bed, expressing herself as very comfortable, and "wishing to know if the tumour had been removed."

The tumour, exclusive of the contents of the cyst, which had been tapped, weighed sixteen pounds; the weight of the fluid contents of the cyst was twelve pounds; making altogether twenty-eight pounds, as the weight of the tumour in its entire state. It was a good specimen of the pseudo-colloid variety of ovarian tumour, so well described by Spencer Wells. An incision into it showed it to be made up of a number of chambers, varying in size from a filbert to that of the clenched hand, and filled with two different kinds of fluid; one kind presenting the appearance and consistency of very thick mucilage, the other resembling prepared chocolate; the parietes of each chamber consisted of a layer of firm fibrous tissue.

I made a careful microscopic examination of the fluid, but could find none of the morbid cells which are characteristic of malignant growth, but only a number of non-nucleated cells, aggregated together, so as to form corpuscles three or four times the size of blood-discs, and having the appearance of fatty granules.

An hour after the operation, the abdomen was covered with a linseed-meal poultice. At 5 P. M., after four hours' sleep, the patient awoke, and expressed herself comfortable, but fatigued, and rather sick. In the evening, she vomited a little,

the effect probably of the chloroform; at 11 P. M., a morphia suppository was introduced into the rectum; pulse, 104; skin, moist. She passed a good night.

Second, third, and fourth days.—Pulse ranged from 104 to 130. The morphia suppository was repeated each evening. She had no pain, no vomiting, and still expressed herself as being very comfortable; tongue, clean. Her diet consisted of Liebig's beef-tea, barley-water, gruel, and tea and dry toast.

On the second day, as the weight of the linseed-meal poultice caused discomfort to the patient, it was replaced by a chamomile-bag, i. e., a flannel-bag filled with chamomile, which had been boiled with water, and freed by pressure from superfluous moisture; the whole being covered with oiled silk, and repeated as often as it became cold. On the morning of the third day, the wound was dressed for the first time; and the dressing was afterwards repeated every day.

Fifth day.—Pulse, 125. At 10 P. M., she was suddenly seized with vomiting so intensely acid as to excoriate the mouth and chin, and so violent as to threaten speedy dissolution; this continued all the night, and the whole of the next day, up to 11 P. M. To afford relief, the following remedies were successively administered: bicarbonate of potash (which, she said, produced a burning sensation in the mouth and throat), carbonate of magnesia with prussic acid, chloric ether with aromatic confection, ice placed in the mouth, an injection of one pint of barley-water to move the bowels (which it accomplished), and, lastly, an injection consisting of one fluid drachm of tincture of opium, in one ounce and a half of water. The cessation of the vomiting appeared to be due entirely to the opiate injection, the preceding remedies having all been ejected from the stomach almost as soon as administered. The relief produced by the opiate injection was immediate, and, in three-quarters of an hour, she fell into a sleep, which continued for five hours. When she awoke, some beef-tea and a little iced brandy were administered; these remained on the stomach; and, in fact, vomiting never afterwards returned. It is necessary to state that, for some time after the operation, the catheter was regularly introduced every four hours.

Sixth and seventh days.—The wound appeared to have united in its deeper portions by the first intention, the superficial portion still remaining open; and there was a slight discharge of healthy pus. One of the pins was taken out.

There was no abdominal tenderness, nor tympanitis; the skin was moist; tongue, clean; appetite, moderately good; pulse, 116; bowels had been slightly moved. The laudanum

injection was repeated at bed-time, in two ounces of beef-tea. From this time she continued to progress very favourably, without any bad symptoms.

Two pins were successively taken out; and on the twelfth day the last pin was removed, the wound rapidly healing. The patient now took port-wine, beef-tea, champagne, brandy-and-water. The laudanum injection was still continued each night. Pulse averaged 104; bowels moved spontaneously once a day.

Sixteenth day.—Ligature came away along with a portion of the sloughing pedicle; pulse was 98; tongue, clean; surface, moist; there was no abdominal tenderness; the wound had healed, with the exception of a small point here and there, and the portion through which the pedicle had passed. The diet was more liberal from this day; the patient taking boiled chicken, mutton chops, and a little sole which she had fancied.

Seventeenth day. Progressing favourably, but complaining of a tickling cough, which was slightly increased towards evening; pulse 104. I prescribed six grains of citrate of iron and quinia, in a little water, three times a day.

Nineteenth and twentieth days. Pulse ranged from 100 to 108; appetite good, skin moist, bowels free; cough still continuing, and, as before, increasing towards evening; for relief, squills, senega, and morphia, were administered.

Twenty-second day. This morning the cough was more urgent; and, in addition, there was a loud mucous r le, owing to a very copious secretion of mucus in the right bronchus and its ramifications, as was evinced by auscultation; pulse small, frequent, thready; the appetite, from having been previously good, now suddenly fell off; profuse perspiration, and symptoms of general prostration supervened, and in the evening she appeared to be rapidly sinking. Beef-tea, brandy, and port-wine, were ordered to be administered. As the patient refused to swallow them, and as the necessity for stimulation was imperative, recourse was had to their free administration "per rectum." This was repeated every hour, in the following quantities:—beef-tea, two ounces; brandy and port-wine, of each half an ounce; mix.

After seven hours, as the patient had improved, the injections were diminished in frequency, but still administered every two hours; the quinia and iron being continued.

On the morning of the twenty-third day, she was much better, but suffered a slight relapse in the evening; the injections were continued at the same intervals during this day, and

her pulse rapidly increased in fulness and strength; the cough, instead of being constant, as heretofore, came on in occasional paroxysms only, the sputum being thinner, and the bronchial tubes clearer; and she was able to take more nourishment by the mouth. The injections of beef-tea and port-wine, however, were continued every two or three hours, as circumstances appeared to call for them, until November 28th, by which time her cough had entirely left her; at this time, also, the wound was healed in its whole extent; her appetite was good, she took solid food at every meal, and was rapidly acquiring strength.

December 2nd. She has continued to progress favourably, and was up to-day for the first time.

From the twenty-second day to the twenty-ninth, the period of the attack of bronchitis, stimulation was resorted to lavishly; the patient taking, by mouth and rectum combined, one quart of old port-wine, and half a pint of brandy, every twenty-four hours, in addition to which, as much beef-tea was given as it was possible to administer.

January 1st, 1861. Since the last report, the patient's progress has been satisfactory, she having been up every day for some hours. A lady, whose residence is situated in one of the most delightful and salubrious of the rural districts surrounding Manchester, moved by the sufferings, and interested by the happy recovery of this poor patient, has offered her the advantage of a month's residence in her house, for the more complete restoration of her health and strength. The departure of the patient for this kind lady's country seat has been delayed only by the prevailing severe weather.

I believe that this poor woman owes the preservation of her life to an adherence to the following principles, laid down by Mr. Spencer Wells, viz.:—

First. In making an incision the smallest possible compatible with the passage of the tumour. I may here remark, that all the cases I have seen in which the large incision, "from sternum to pubes," was used, proved fatal.

Second. In bringing and securing the pedicle, external to the incision, instead of leaving it to slough (as was the practice heretofore), within the abdominal cavity. It has fallen to my lot, on more than one occasion, to witness the death of patients about the third week after ovariectomy; the result, as there is the strongest possible reason to believe, of pyæmia,—the abdomen, on a post mortem examination, having been found filled with pus, which could be traced to no other cause than the sloughing pedicle.



Third. In the use of silver pins passed through the abdominal parietes, so as to include the peritoneum, thus securing union, by first intention, of the deep parts of the wound, including the divided edges of the peritoneum; and so, by closing the peritoneal cavity, securing it against the entrance of pus, which might otherwise fall into it from any superficial suppurating portions of the wound.

In my opinion, I believe, also, that part of the successful result is due to the *very free* administration of nutriment and stimulants, "per rectum," when the patient was unable or unwilling to swallow them, and at a time when death appeared imminent from the want of them.

I would draw the attention of my professional brethren to the remarkable extent which nutriment and stimulants are capable of being absorbed into the system when administered by this channel.

Great relief was also afforded by the substitution of a light chamomile-bag, covered with oiled silk, instead of the heavy linseed poultice; the latter having occasioned the patient much discomfort by its weight. I have already mentioned that the opiate injection was the only remedy which was successful in checking the obstinate vomiting.

In conclusion, I would wish to take this opportunity of recording the praiseworthy manner in which the patient was cared for, in my absence, both by day and night, by Mr. Runcorn, House Surgeon, and Mrs. Horncastle, Matron, of St. Mary's Hospital, whose indefatigable exertions have done much in bringing this case to a successful termination; the satisfaction derived from the remembrance of having so conducted to it being the sole, but ample reward, of their sacrifice of time, labour, and health.

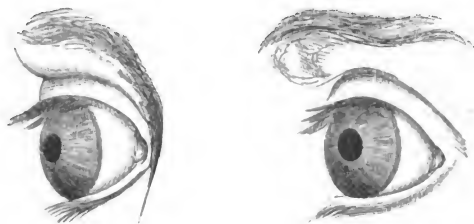
ART. IV.—*An Essay upon the Malformations and Congenital Diseases of the Organs of Sight.* By W. R. WILDE, M.D., M.R.I.A., F.R.C.S., Surgeon to St. Mark's Ophthalmic Hospital, Honorary Member of the Medical Society of Stockholm, &c. &c. With Illustrations. Part III.

(Continued from vol. vi. p. 289.)

CIRCUMSTANCES have prevented my continuing those articles upon the malformations of the eye, which, the readers both of the former and the present series of this Journal may recollect, were commenced as far back as the year 1845. By reference to the number for November, 1848, it will be seen that

I concluded the article of that date with a description of the malformations of the iris and choroid. According to the plan originally adopted in the arrangement of this essay, we now come to the consideration of the congenital diseases and malformations of the dioptric media. The abnormal conditions of the fluid filling the aqueous chamber, irrespective of general disease of the organ, such as hydrophthalmus, must be very rare in the human eye. I am not aware of any well-authenticated instance mentioned by the various authors whom I have consulted, of its being in greater quantity than natural, except that related by Benedict and Demours. Von Ammon mentions the instance of a calf, in which the anterior chamber was filled with a dirty red, gelatinous mass, and there was also a thickening of the membrane of aqueous humour; and Professor Prinz afforded Seiler the particulars of the case of a dog, in which, at birth, a movable body, with a polished surface and a sooty appearance, was found in the anterior chamber; but, as the author himself observes, it was, probably, an opaque lens, marked by the pigment of the uvea, which had started through the pupil<sup>a</sup>.

Dr. France, of London, has recorded a case of congenital deficiency of the aqueous humour, and has been good enough to furnish me with a drawing of the patient, from which the accompanying illustration has been made. The corneæ were slightly convex, the irides bluish, and lay in contact with the



corneæ. There was no anterior chamber, and no aqueous humour; the irides were active, and free from adhesions; the pupils blue, clear, and circular. The subsequent history of this case, forwarded to me in 1850, is unknown<sup>b</sup>.

#### MALFORMATIONS OF THE LENS AND CAPSULE.

The best authorities upon embryology, and more especially upon the formative process of the human eye, state that the

<sup>a</sup> Seiler's *Beobachtungen ursprünglicher Bildungsfehler und gänzlichen Mangels der Augen*. Dresden, 1833, s. 61.

<sup>b</sup> London Medical Gazette, vol. x., p. 11.

lens becomes apparent in the human fœtus about the commencement of the sixth week. Minute anatomists differ as to whether the capsule, with its contents, is a portion of the original internal or medullary tube, continued on into the ocular vesicle destined to form the future visual organ, or a reflexion of the external tegumentary envelope, drawn inwards so as to form the lens-capsule, and afterwards nipped off by the action of the iris and choroid.

It would not tend to elucidate the more immediate object of this essay, were I to enter into a discussion of the various and rival theories which have been promulgated during the last twenty-five years upon this subject. As already stated in the former portions of this essay, the *Encyclopédie Anatomique*—especially vol viii., containing Bischoff's *Entwicklungsgeschichte der Säugethiere und des Menschen*, translated from the German by Jourdan, published in 1842—really contains the great bulk of all the authorities which could be quoted upon the subject. Some anatomists include the vitreous substance with the original formative process of the lens; and others define it as a mere mechanical recipient of that body. The former, however, seems the most plausible theory, and one justified by the subsequent intimate relation of these two structures. Huschke, one of the earliest and most accurate observers, while he believes in the connexion of the vitreous body with the primary medullary tube, holds that the lens is formed by a reduplication, or tucking-in of the tegumentary membrane. Those who assert that the lens-capsule is formed from without, produce, in proof thereof, the fact of the original want of chambers in the eye, and also, that at an early period of embryo existence the anterior section of the capsule is in immediate contact with the iris, and, so far as the *membrana pupillaris* admits, with the cornea also. It is even asserted that an opening exists (in the chick, for example, at the end of the third day) in the middle of the future cornea; but this has been denied by others. The substance of the lens is believed to be formed by, or secreted from, the inner layer of the capsule; and in the early period of fœtal life is composed of a mass of granules and globules of a much softer consistence than it subsequently assumes, when these globules, uniting in compressed lines or bands, form the fibres of which the future lens is composed. In consistence it is then soft, compared with all its future stages from birth to old age, and is of a reddish hue; but whether derived from the amount of surrounding vascularity (as from the capsule and the reddish vitreous body, &c.), or by any deposit of colouring matter within its substance, has

not been clearly defined by writers. Some say that the lens is originally opaque, while others assert that it is transparent. These are questions of much interest in investigating the causes of those peculiar appearances which present at birth, and form the more immediate subjects for description in this essay.

During the first months of fœtal life the lens is globular, like that in fishes, and larger in proportion to the size of the globe of the eye than in extra-uterine life. According to Arnold, the process of becoming transparent begins in the circumference, and proceeds towards the centre; and this is an opinion in which, from my own observations upon congenital cataract, I can fully concur. The late venerable Von Walther of Munich also maintained the original opacity of the lens, and asserted that cataract was an arrest of development while it was passing into the normal transparent stage; while Von Ammon of Dresden, of whose observations I have so largely availed myself in the foregoing portions of this essay, maintains that it is a pathological change in an originally transparent structure. About the period of birth the lens becomes relatively smaller, and less spherical, than in the fœtus.

The capsule, resembling in structure the elastic lamina of the cornea,—a description of transparent fibro-cartilage,—may be said to be that which gives form to the lens, as well as produces from its inner secreting surface the proper substance of that body. It also supplies it, in early life, with its proper vascular support, by means of the ramifications of the *arteria centralis retinæ* upon its posterior surface, and a venous connexion anteriorly with the *membrana pupillaris*; while upon its periphery, and for some distance over its anterior edge, a minute and intimate vascular connexion is maintained with the edges of the ciliary process. Judging from analogy, we may well suppose that the membrane of aqueous humour, which passes over the front of the capsule, affords a general vascular connexion to all those structures, both in the anterior and posterior chambers, with which it maintains anatomical relations. Intimate as these connexions of the lens with the surrounding parts of the eye appear to be in embryonic and fœtal life, they cease to be demonstrable after the full period of intra-uterine existence; so that, except by the fact of an adhesion of the lens to its capsule, when the latter is fully opened, we have no anatomical proof, either by injection or by the aid of the microscope, of any union between these two parts.

“As the crystalline lens in the healthy eye of an adult,” says Dalrymple, in his “*Anatomy of the Human Eye*,” “is perfectly transparent, and void of colour, it follows, of course,

that it contains within its substance no vessel carrying red particles of the blood; and inasmuch as in earlier periods of the science the art of making anatomical injections was unknown, it is anything but surprising that the existence of bloodvessels in this part of the eye was denied by Galen and the old anatomists. Haller, in his description of the arteries of the eye, quotes an Englishman of the name of Allen Moulin as the first observer, and in fact the discoverer, of these long-denied vessels." These vessels were, however, subsequently acknowledged by Ruysch, and demonstrated by Winslow. The "Englishman" alluded to in the foregoing paragraph was Allen Moulin, or Mullen, an Irish anatomist, and one of the original members of the Dublin Philosophical Society in 1684\*.

Ancillary to the foregoing observations may be considered the question of the restoration or regeneration of the eye, in whole or in part, as related by authors. The experiments and observations which have been made on the lower animals tend to prove that the contents of the capsule may be repaired after its removal or absorption. So early as 1781, C. Bonnet appears to have made experiments which proved that the eye of the newt, or salamander, could be restored, if the globe, as far only as the entrance of the optic nerve was removed<sup>b</sup>. Blumenbach made some observations upon the same subject subsequently<sup>c</sup>. In 1801, G. Vrolik, of Amsterdam, observed that a depressed lens had been imperfectly replaced, after ten years, by an irregular ring of lenticular substance, open in the centre, and which he thought arose from a new secretion of lenticular substance, owing to the incompletely-destroyed vessels of the capsule<sup>d</sup>. Twenty-six years later, Leroy D'Etiolles, whose experiments were chiefly made upon rabbits, cats, and dogs, and who was, perhaps, unaware of the foregoing discoveries of Bonnet and Vrolik, stated that the restored part was either a crumbly mass, a diminutive, lenticular body, or even a full-sized lens. Mayer's experiments demonstrated that in rabbits, eight days after the destruction of the lens, a small ring of soft lenticular substance had formed; and that this went on increasing in quantity until the middle of the fifth month after the operation, when a new lens, open in the centre, but normal in the circumference, was formed; but although it is not so stated, these so-

\* See the Author's Preface on the History of Medical Literature, in the first volume of this Journal.

<sup>b</sup> *Œuvres d'Hist. Nat. de Philos.* Liv. xi., 1781.

<sup>c</sup> *Kleine Schriften* Leipzig, 1804.

<sup>d</sup> See Müller's Physiology, translated by Baly; and also Magendie's Journal for 1827.

called lenses were, I presume, opaque. When the periphery of the capsule was destroyed, there was no reproduction.

In the details of all these experiments, the original mode of destroying the lens is not clearly expressed. The subject has been much discussed among the German physiologists; but the results of their inquiries and experiments have not tended to advance practical science—in this country, at least. William Soemmering's and Werneck's experience confirm the opinion of Vrolik, and show that after reclination, or depression, a crystalline mass is sometimes formed in process of time within the capsule, the posterior segment of which is alone open.

As, however, almost all these experiments and observations have been recorded by anatomists not practically engaged in operative ophthalmic surgery, and consequently not very familiar with pathological products in the human eye, I think I am warranted in asserting that we require satisfactory evidence as to the reproduction of the lens, in whole or in part, in man, under any circumstances. Those who have had much experience in operating know full well that, owing to the cortical substance of the lens being softer than the nucleus, especially towards its circumference, a portion of it often remains within the capsule, even after the operation of extraction. Unless anatomists are also experienced operators, their mode of treating the eye in the lower animals for the purpose of these experiments, may, I think, with justice, be questioned by those who well know the great difficulties which surround this branch of practical surgery. Even acknowledging to the fullest extent the most favourable results of these experiments, observations, and assertions, no practical good is ever likely to accrue therefrom, no more than from the asserted transplantation of the cornea, or, as stated, the reading with the pit of the stomach, as some years ago was gravely stated to me by an eminent dignitary of the Irish Church\*.

Deviations from the normal condition in this part of the organ of vision may be divided into:—the absence of lens; the

\* Vagaries of this description, so repugnant to common sense, irrespective of science, and so subversive of the cause of true religion, might be laughed at in the ignorant, and despised in the charlatan; but when they are upheld by the exalted in station and influential in position, they become truly dangerous; and when they bias a mind, by nature "open as day to melting charity," so far as to withdraw from the public charities of a large city, which supply shelter, food, care, and all the appliances which science and humanity have devised for the amelioration of human suffering, because mesmerism and homœopathy are not practised in such institutions, then there can be no hesitation in asserting that such conduct becomes not only deplorable, but reprehensible.

fusion of two lenses together, as in monocoli; a double lens in one eye; malpositions of the lens, and alterations in the size, form, structure, consistency, and transparency of the lens and its capsule, as well as organic changes, such as absorption of the lens, and adhesions between the iris and the anterior capsule, from intra-uterine accident, or disease; and, finally, entozoa within the capsule, &c.

*Absence of Lens.*—Rosas quotes Morgagni, Von Walther, and Arnemann, as authorities for cases of deficiency of the lens<sup>a</sup>; and Haller records the case of a child with but one orbit, containing, however, two globes, but not fused, as in monocoli. One globe was perfectly normal; in the other, however, the lens was absent<sup>b</sup>. Seiler has recorded a case of deficiency of the lens in a microphthalmus. A similar case is recorded by Von Ammon; but it is probable that in both instances the deficiency was the result of the general disease, or arrest of development in the whole organ at a very early period of foetal life. In case of deficiency of lens, one would expect fluctuation and unsteadiness of the iris.

*Double Lenses.*—Supernumerary lenses have been observed by Vallisnieri and Fritsch; but the particulars are not specified in the works to which I have had access; and it is possible that these cases may have been instances of cyclopia, in which two eyes were fused together. Wardrop says—"The crystalline lens is sometimes entirely wanting; in which cases the pupil is also double"<sup>c</sup>. But he does not cite his authority, nor more particularly describe the cases. He adds, however, that Heister "found it [the lens] divided into different portions," or fused lenses. See *Monocoli*.

*Myopia*, arising from abnormal curvatures of the anterior or posterior capsule of the lens, must frequently be the natural or born condition of these parts, although not remarkable until the child has arrived at that period of life when its short-sightedness attracts attention. Authors state that congenital cataract has been confounded with, and occasionally mistaken for, myopia. I have more than once seen such a case. Porterfield says the lens has been, in some rare instances, unusually flat; and hence the patient was congenitally presbyopic, or long-sighted. And Foigtel states that it has been found of a triangular shape.

*Malposition of Lens.*—Klinkosch asserts that he saw a lens

<sup>a</sup> *Handbuch der theoretischen und praktischen Augenheilkunde von Anton Rosas.* Wien, 1830, I. Band Seite, 284.

<sup>b</sup> *Hist. de l'Acad. de Science*, 1751, p. 49.

<sup>c</sup> Essays upon the Morbid Anatomy of the Eye, by James Wardrop, vol. ii. p. 76.

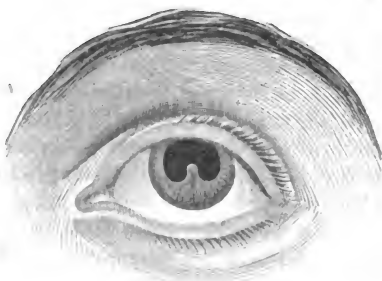
behind the vitreous body, and lying on the optic nerve. Such a case is not by any means improbable, when we consider how easily it might occur in case of fluidity of the vitreous humour. Although anterior dislocation of the lens, observed at birth, has not been related by authors, it is an accident so likely to happen, that one rather wonders at the absence of it than otherwise.

Mr. Dixon, in the last edition of his "Guide to the Practical Study of Diseases of the Eye," records the circumstance of the "abnormal position of the lens occurring in four members of the same family," in which the lenses appeared to have had one edge tilted forwards, and the other backwards, either from side to side, or from above downwards. In one instance, however, the twist of the lens took an oblique direction<sup>a</sup>.

*Entozoa within the Capsule* at birth have been observed and described by several authors. Von Ammon has figured two forms of *Distoma*, and also a *Filaria oculi humani*, found in connexion with congenital cataract; but does not state the particulars. Nordmann found entozoa between the lens and capsule in the embryo of fishes. The same author, Gescheidt, and other observers, have related many cases of entozoa within the capsule after operations performed for non-congenital cataract<sup>b</sup>.

The lens has been found absorbed, and the capsule shrivelled up, apparently from defective nutrition, during embryo life.

*Congenital Synechia Posterior*, the result of intra-uterine iritis, occasionally occurs, as I have shown in the previous portion of this essay. It is a pathological condition which I have myself observed so soon after birth, that it could only be attributed to inflammatory, and probably specific action going forward in utero. The case illustrated by the accompanying woodcut is, however, of a different character from those instances of syphilitic intra-uterine iritis which are usually met with. The pupil, here represented under the influence of belladonna, is horse-shoe-shaped, owing to attachment of



<sup>a</sup> See Appendix to the Second Edition, p. 401.

<sup>b</sup> Upon this subject see *Zeitschrift für Ophthalmologie*, band III. heft 4; and article "Entozoa," in the *Cyclopædia of Anatomy and Physiology*, by Professor Owen; also *Archiv für Ophthalmologie*, Bd. i., iv., vii.



a small portion of the free edge of the iris to almost the centre of the lens-capsule; this appearance existed at birth. Vision was quite perfect when I had this drawing made, in 1849, from a young woman then aged 23.

*Congenital Cataract* may be divided into capsular, lenticular, and capsulo-lenticular, according to the structures affected, —partial or complete, according to the amount of opacity present; fluid, or of the normal consistence of the lens; and single or double, as one or both organs are affected; in all of which this disease is analogous to those forms observed in after life. But there are two descriptions of cataract which are solely the result either of intra-uterine disease, or that occurring immediately after birth; these are the central and the pyramidal.

Until the days of Saunders, English surgeons do not appear to have paid much attention to congenital cataract, neither had it been well described by Continental authors. To Saunders is also due the introduction of the early anterior operation for this affection, or what he properly terms the operation on the capsule. Since then, German writers have been very minute in their definitions of congenital cataract; thus, Himly describes *Cataracta capsularis totalis, anterior, posterior, et centralis; C. lenticularis dura, caseosa, lactea, et centralis; and C. capsulo-lenticularis, dura, centralis, et pyramidata*. Among the forms of cataract described by Von Ammon, including all the foregoing, he enumerates those connected with the malformations of the iris, club-foot, umbilical hernia, and elongated crania, &c.

The most frequently attending symptom of congenital cataract is nystagmus, which is more or less decided, according to the density of the opacity, or the space it occupies behind the pupil, and occasionally aggravated by amaurosis. The two forms of motion of the globes may, however, be distinguished by a careful and experienced observer. Where the irregular motion of the eyeball has been caused or aggravated by nervous disease, there is generally a staring look in the eye, and the motions partake more of the rotatory form than those more side to side; while in the uncomplicated cataract, with a healthy brain and retina, the movements consist of a "peering" upturn of the eye, at the same time that the features of the face are brought into what may be termed a seeing or listening attitude; occasionally, the globes take sudden transverse sweeps within the palpebral aperture, as if seeking light, while the head is usually turned a little to one side. According to my observation of congenital cataract, it is very rarely connected with any other malformation. In two instances, however, both in men past middle life, and affected with double congenital cen-

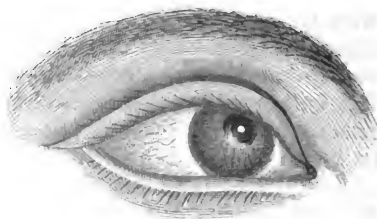
tral cataract, each consisting of a dense, white spot, about the size of a large pin's-head, there was a remarkably high formation of skull,—what the Germans would call “Spitzkopf,” and not unlike some of those malformed heads figured by Von Ammon, in his tables of *Cæcitas Congenita*.

I lately witnessed a case of congenital cataract, with deficiency of the iris on both sides; but as the girl had been operated on prior to my seeing her, I cannot state what was the original condition of her lens and capsule. She had intense nystagmus, and great intolerance of light. I am inclined to think such cases should not be interfered with. Tremulous iris occasionally coexists with the born cataract, and the lens itself is occasionally shakey.

The causes of congenital cataract may be divided into the proximate and remote. The latter is more frequently induced by hereditary predisposition than any other affection of the eye with which I am acquainted. I also believe that it may, like muteism, be induced by the consanguinity of parents; but we require more extended observations to confirm this opinion. All authors, however, agree with respect to the hereditary character of the disease. I saw three generations affected with it, and I operated upon members of two of them. All the older authors, from the days of Maitre Jean and Woolhouse, were well aware of the hereditary nature of this disease. The journals and works upon ophthalmology record many curious instances, illustrative thereof. I have several times seen three of a family affected with it; and I know an instance in which the father and his six children were born blind from this cause. The father had been operated on by the late Dr. Little, of Sligo; the three children whom I examined had all malformed eyes, in addition to the opacity of the lens; two were totally amaurotic, and the third had a microphthalmus on one side. The form of cataract, whether central, lenticular, or diffused, &c., is generally persistent in the different members of the family, although I have seen exceptions thereto. Saunders relates the following remarkable case, showing the identity of the disease in the same family:—“Two brothers, twins, became blind with cataracts at the age of twenty-one months, each within a few days of the other. It is remarkable that the four cataracts had precisely the same character.” In these instances, however, it is more likely that the children were born with cataracts.

Before I come to discuss the proximate cause of the diseases, I wish to dispose of one form of non-congenital cataract, with which it is often confounded. Under the head of congenital cataract, authors have described central capsular cataract, caused

by ophthalmia neonatorum; but they have not explained the mode in which this occurs. Infants become affected with purulent ophthalmia a day or two after birth; in many cases the cornea opens in the centre, thereby differing in some respects from the violent ophthalmia of adults, in which that structure most frequently gives way near its circumference. Possibly, the cornea is weaker in the centre at birth than subsequently. The rent having taken place, either by slough, ulceration, or what might really be styled a crack or burst, the aqueous fluid is evacuated, and the iris and lens fall forward against the cornea, where the capsule, by simple apposition with an ulcerated, or otherwise diseased surface, becomes, in my opinion, inflamed and opaque by mere contiguity of parts. The rent in the cornea closing, by lymph-deposit on its edge, or becoming plugged up with a portion of protruded iris (in which case there is permanent synechia anterior), and the fluid re-accumulating, the chambers are re-formed, and the lens is restored to its normal position, but with the central opacity remaining permanent. It is always in one eye only, and is often connected with the anterior synechia, as shown in the accompanying illustration, taken from a man then aged 25, and with whose history I am acquainted.



The proximate causes of opacity of the lens may be stated as arrest of development, during the clearing process, supposing the lens to have been originally opaque—interstitial deposit of opaque matter occurring in utero as in after life—inflammation producing opacity especially of the capsule; and morbid growth, as in the case of anterior pyramidal cataract. According to Arnold, the lens-substance is originally a thick milky fluid, which subsequently acquires the consistence of albumen, then becomes more solid, and finally begins to clear at the circumference, leaving the central nucleus opaque, as in the fifth week in the foetal calf, and does not altogether become transparent till the eighth week.

Werneck found the human lens opaque up to the eleventh, and in some instances to the fourteenth week. Thus, an arrest of development at the very earliest stage of embryonic life would afford at birth the milky cataract; and, at a little later period, that of the general homogeneous opacity which is a

common form of congenital cataract; although, I believe, it may also be the result of inflammatory action, or congestion. The vices of formation receive more distinctive proof from that form of cataract which Dr Mackenzie calls *cataracta cum zonula*, in which there is a central nucleus of opaque matter surrounded by an envelope of transparent lens, through which occasionally run spiculæ, or needle-shaped opacities. Other authors, and among them, Haller, Zin, Petit, Seiler, and Von Ammon, hold that the lens is originally clear, and that the deviations from the normal formative process are owing to disease in the capsule or lens, and more particularly to obliteration of the arteria centralis retina. This is susceptible of explanation by analogy with a not uncommon accident in after-life—concussion of the lens from a blow on the eye, the brow, or the temple, when the surrounding vascular connexions of the crystalline being destroyed, it becomes, in an incredibly short space of time, opaque. But although this explanation may account for the diffused form of lenticular cataract at birth, it does not elucidate the circumscribed and partial opacities of congenital cataract. Scrofula is also charged with this congenital defect; but in what way it acts has not been explained by the supporters of that doctrine. In an inquiry of this nature, nothing very certain or exact can be affirmed: and, even if it were, it could exercise but little practical effect. The forms, however, which the disease presents are of far greater importance to the ophthalmic surgeon.

It is asserted by Wardrop and other writers, that although congenital cataract commonly affects both eyes in the same way, that some times the lens of the one and the capsule of the other is affected; but this is contrary to my experience; for where the cataract was truly congenital, I have always observed a marvellous amount of symmetry between the eyes. It is true, a slight speck on the anterior capsule may be observed in connexion with lenticular cataract in one eye, and not in the other; but it is not sufficiently extensive to be denominated capsular cataract, according to the sense in which it has been applied by Wardrop. Having paid a great deal of attention to this subject many years ago, I had a very extensive collection of drawings illustrative of the different forms of the disease (many of which are not figured in books), made by Mr. Connolly and the late Mr. Neilan; but without coloured engravings, it is very difficult to represent their special peculiarities.

Distinguished from all other forms of cataract, the congenital, which is the result of some vice of formation, almost invariably presents a definite, distinct, well-defined figure, not

shading off gradually into the surrounding clear portion of the lens, as in the opacity of age, nor mottling the lens irregularly, like that of middle life, but presenting a sharp line of demarcation between the opaque nucleus and the surrounding normal transparent structure.

Lenticular congenital cataract may be divided into the complete and partial.

*Complete Lenticular Cataract* not uncommonly presents at birth, in which the capsule is unaffected, and the entire substance of the lens is of a uniform, skim-milk colour, homogeneous, and retaining, so far as we can judge, something about its normal consistence, perhaps a little softer. I have seen this transmitted more than any other form, although it may occasionally appear accidentally in one member of a family. Owing to the great occlusion of light, there is always nystagmus in such cases; and they are those which usually require early operation. Von Ammon relates the case of a hard lenticular cataract in one eye, and a soft lens, with partial opacity of the capsule, in the other. I have seen one instance (to be described hereafter) in which the lens was yellowish, and apparently hard in one eye. In two cases of congenital cataract, also recorded by Von Ammon, the yellow spot on the retina was wanting—as shown, I suppose, by dissection.

*Cataracta Lactea*, in which I presume the lens has either remained in its original fluid and opaque condition, as already stated, or, having become solid and opaque, has degenerated into the milky substance observed at birth, like the softening process which the cortical substance of the lens undergoes in what is termed Morgagnian Cataract, is rather rare. In all the cases of this kind which I have seen, the lens appeared unusually large; there was great nystagmus, and a certain amount of amaurosis, with dilatation of the pupil, present; the capsule is frequently mottled with greyish-white spots. This form may be distinguished from the foregoing variety by its less apparent density; and, when the capsule is mottled, by the greater amount of relief with which such opacities are thrown forward. But it requires a very practised eye to make an accurate diagnosis of the amount of fluidity in certain cases of cataract. One of the most remarkable circumstances attending this pathological condition is, that while the colouring matter of this milky substance remains either well mixed or suspended in the fluid, so long as the capsule is intact, when it is opened, as by the needle in operating, and that the whitish substance mixes with the contents of the aqueous chambers, it gradually, but within a few minutes, deposits in

the form of a greyish-white powder, leaving the pupil perfectly clear; as if the admixture of the aqueous humour had chemically acted upon it, and thrown down the precipitate referred to. Occasionally the substance which is deposited in the bottom of the anterior and posterior chambers produces, like that observed in Morgagnian Cataract, great constitutional disturbance, with irritability of stomach, and inflammation of the eye.

Neither of these two forms of cataract could be exhibited effectively in a woodcut.

*Central Lenticular Cataract.*—I have been for many years acquainted with a peculiar form of congenital cataract, to which this appellation is applicable; and I presume it is that known to some of the older writers as the *cataracta cum zonula*, but which name Dr. Mackenzie applies to that form of siliquose capsulo-lenticular cataract, in which the lens and capsule, “not having grown in proportion to the rest of the body, but remaining nearly of the size they were at birth, on dilating the pupil, a black zone—formed exteriorly by the ciliary processes, and interiorly by the space between them and the circumference of the capsule—is brought into view, surrounding the cataract.” The disease, however, of which I speak, is purely lenticular, and unattended with any alteration in the form or deficiency of size in the crystalline. It is so frequently met with in this country, that having a large collection of drawings of both eyes (in 13 cases), made of it many years ago, and representing all its varieties, I have long ceased to illustrate such cases when they present. The only author who has presented us with anything like a faithful representation of the disease is Sichel, in his *Iconographie Ophthalmique*, pl. xviii. figures 1 & 2. In Dalrymple’s *Pathology of the Human Eye*, Plate xxvi, figures 4 and 5 would appear from the drawing to be of this nature; but the author, no doubt looking upon the central opacity as including the entire substance of the lens, believed that the crystalline was “manifestly smaller than the natural structure”—an opinion to which Sichel also leans.

The accompanying woodcuts serve, as well as such illustrations possibly can, to illustrate this form of the disease, but they fall very far short of giving anything like a characteristic expression of the appearances. On looking into an eye so affected, we observe an opacity, more or less dense and whitish, behind the pupils; but at such a distance, and with so much shadow thrown from the pupillary edge of the iris, as to show a practised eye that a considerable clear space intervenes between the level of the pupil and the surface of the opacity. On

dilating the pupil, we then observe a clear, sharp, well-defined, and always circular edge to this opacity, round which there is a perfectly clear, and consequently black zone, varying in size according to the magnitude of the central opacity, which usually occupies about three-fifths of the transverse diameter of the normal lens—the entire cortical substance of the lens, both in its antero-posterior and transverse diameter, together with the capsule, remaining perfectly clear. To an eye long accustomed to such examinations, this piece of opaque matter presents somewhat the appearance of the coloured enamel seen in the centre of some of the specimens of old Venetian glass, an art latterly revived in our globular paper-weights. The patient always experiences considerable improvement from the dilatation of the pupil; and it is a description of cataract which, when in a mild form, is so frequently mistaken for myopia, that it is often first observed in adults, having been in early life attributed to that affection\*.

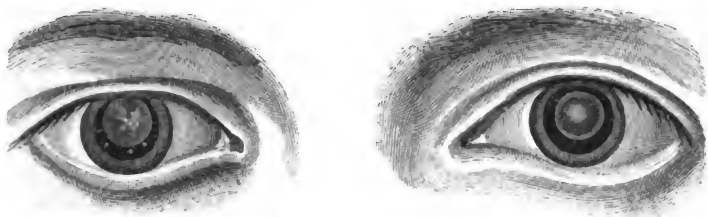
In the early cases of this disease which I met with, I naturally experienced some difficulty in diagnosis, but the first on which I operated, proved the correctness of the opinion I had formed; for as soon as the capsule and anterior layer of the lens were well incised with the cutting edge of the needle, the latter became, in a very short time, opaque throughout, as in ordinary cases of traumatic cataract<sup>b</sup>.

This form of cataract is often complicated with opacity of the capsule; and the disease itself, in its uncomplicated form, presents many varieties. The opaque nucleus is seldom uniform in colour and density throughout, although I have seen it occasionally so. The edge is sometimes whiter, and apparently denser than the portion immediately within it, and occasionally presents a very remarkable whitish ring. With this form, as shown in some of these illustrations, there is a central

\* Several years ago I operated upon two ladies and a gentleman in one family, all upwards of twenty years of age, where the disease had not been discovered until the time I saw them. Their defect of vision was attributed to hereditary shortsightedness—their mother having very defective sight. Not long since, while examining a lady's eyes, I was struck by the peculiar moping gait and peering look of her daughter, a girl of seventeen or eighteen years of age, who was in the room; and, on inspection, I found she had congenital cataract in both eyes. On asking the parents about her, I received the following answer: "Oh yes, indeed, poor thing, it is a great pity; she's very blind, and we have gone to a great deal of expense in trying to get her proper glasses." The poor girl herself complained bitterly of the treatment she received at school, on account of her "stupid blindness." Neither the school-mistress nor the parents had ever thought of having her examined by any competent authority. She has since continued to use atropine, and derives so much comfort from it, that she is unwilling to risk an operation.

<sup>b</sup> Persons about to submit to an operation for this form of cataract should be informed previously of the result of the first necessary step towards solution.

spot denser than the portion between itself and the ring; and occasionally this part assumes a very regular figure, as seen in the right eye of William Nightingale, a boy aged 18, from whom I had this drawing taken, prior to operation, in 1847.\*

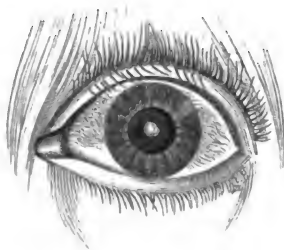


Sometimes, instead of the appearances there presented, we may observe a little nucleus of white dots, each about the size of a small pin's-head, and which may be distinguished from the surrounding skim-milk colour of the opaque portion of the lens, as shown in the accompanying illustration of Mr. K.'s eyes.



In one instance the spot was so small, white, and central, as to resemble the cataracta centralis, which has its seat in the anterior capsule of the lens; and it was only by examining the eye in partial profile, that the difference could be distinguished. In two cases, I have seen lenticular cataract of a straw-coloured yellow, with a central anterior knob of dense white, like chalk. Both instances were adults, and the lenses were possibly hard; but the defined form of the opacity luted into the centre of the clear lens in each left no doubt as to its congenital origin.

In the accompanying illustration, drawn from Bridget Danass, a woman, aged 46, both the draughtsman and engraver have been particularly fortunate in hitting off the aspect of the disease, and in showing the spaces that exist between the edge of the iris and the central nucleus of yellowish opacity of the lens on the one hand, and on the



\* These blocks, as originally engraved, being too wide for the page, have been cut in the middle.



other the chalky-white anterior central cataract within the capsule, which throws its shadow upon the yellow opacity behind it. In the remainder of the lens, the interspace between the capsule and the congenital cataract was clear. The upper lid was affected with trichiasis, as shown in the illustration. It was for the latter disease the patient came under my observation.

Not an uncommon variety of this form of partial lenticular cataract consists in white striæ, radiating from a central spot with



extreme regularity, as shown here in the eyes of Louisa Pen-tony, aged 12, who was operated on in 1849. I may here mention, that regularity of formation affords, in most of these cases of congenital cataract, a very ostensible differential diagnosis.

Starting from the edge of the opacity, we occasionally perceive a number of white lines passing out to the circumference of the lens, through its clear annulus, not unlike those spiculæ of opaque matter which are often the first manifestations of cataract in after-life, and are chiefly situated on the posterior surface of the lens, having their base at the extreme edge, with their apices pointing to the centre. Like these, the radiating lines in the periphery of the lens in congenital cataract, chiefly occupy its lower half, although a few are occasionally seen all round. Sometimes the radiating lines of the cataract are continued on into those peripheral ones; and in the accompanying drawing we see, in addition thereto, three opaque spots very distinct, and occupying the clear peripheral space in the normal lens. In his description of Plate xxvi, figure 4, already alluded to, Dalrymple has given a figure of what would appear to be a partial central opacity of the lens, although he was evidently aware of its surrounding normal investiture. In that repre-

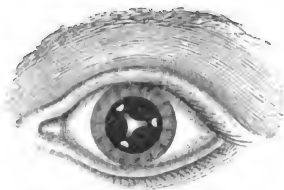
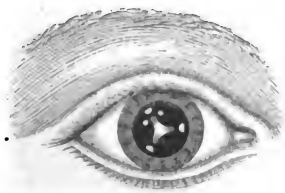


sensation, we observe two of those radiating lines, upon which he observes—"Sometimes we see a little opaque tag or fibre, which seems to attach the capsule to the vitreous body, or, more probably, to the anterior layer of the zone of Zinn; as if, the lens not increasing with the general growth of the rest of the eye, its former natural connexions had been partially separated, leaving, here and there, points of adhesion, which retain it in its central position." From our knowledge of the pathology of the eye, we could scarcely expect to find a dwindled lens within a clear normal capsule. The foregoing is one of the very few instances in which that observant author failed to detect the true character of any appearance within the human eye.

In connexion with central lenticular cataract in the left eye of Anne Duffy, aged 13, the appearance shown in the accompanying cut was observed when the pupil was artificially dilated. It resembled a curtain, with a fringed up-right edge, stretched across and behind the iris. It was neither a development of the ciliary process nor a persistence of the membrana pupillaris. In colour it was light brown below, and whitish at the fimbriated margin. I have not seen the case since the drawing was made, in 1846.



When I commenced the revision of this portion of my essay, I thought I was acquainted with every form which congenital cataract could assume; yet here is the appearance in the eyes of a child, aged 4 years, who was brought to me last



week, and which, so far as I know, has never been observed previously. In the centre of each lens there is a whitish cruciform mass, the outline of which presents an extraordinary amount of regularity. Towards the extremity of each limb of this

figure, and somewhat deeper in the clear substance of the lens, may be seen small triangular pin-head opacities, and near the upper and outer limb on the right side, there are three white specks. Vision is imperfect, but the child is well-formed and healthy.

Very rarely, indeed, do we observe a small circular opacity in the centre of the lens; and when we do, it is very difficult to give it the proper expression.

The accompanying drawing is the nearest approach to it which an illustration in wood can effect. Seiler says, that although the central cataract is usually white throughout, he has seen it annular, with a transparent



spot in the middle; and he has figured this appearance in the plate attached to his work already referred to, see figure xi.

*Posterior Lenticular Cataract.*—Opacities upon the posterior capsule of the lens, if not often seen by some of the older authors, were at least believed to be of frequent occurrence. Many years ago, I observed a yellowish spot, with a few radiating fibres, very deep, in each lens, in a middle-aged man. I had a very careful drawing made of the appearance which the eyes then presented. In the course of about three years, the entire body of each lens became opaque. I extracted them, and demonstrated to those present upon each occasion, the opacity upon the apex of the posterior surface of the lens; and, when the section of the cornea had healed, showed that there was not the slightest opacity remaining upon the capsule. The man worked many years subsequently as a shoemaker. Since then I have seen many such cases, and never failed to recognise them as diseases of the posterior surface of the lens.

M. H., aged 15, with impaired vision from birth, presented the following appearance:—A dense brownish-yellow opacity,



circular in shape, larger in the left eye than the right, occupied the apex of the posterior surface of the lens. Surround-

ing each, there was a slight nebulous condition, with a well-defined edge not easy to represent on wood. The operation for solution was performed in both eyes, without any untoward circumstance; but although he is greatly improved, and now able to follow an out-door occupation, he is not able to read, and has derived less assistance from glasses than any case of congenital cataract I have ever met with.

A very natural suggestion here presents itself, and one which is sure to be asked by the partially blind from congenital cataract, or their friends—Will the disease increase, either in density or extent, unaided by any adventitious or accidental causes—such as congestion, inflammation, age, &c.? I believe not. I have never seen, nor have I read of, a well-authenticated instance of congenital opacity of the lens, or its capsule, increasing in after-life. Dr. Mackenzie, in the last edition of his great national work on the diseases of the eye, at page 769, when discussing the question of operating early in cases of congenital cataract, says—"If the operation is delayed, the capsule becomes opaque and tough, and, therefore, much more difficult to remove, &c." This opinion is contrary to my observation and experience of the disease. I never saw a capsule, unaffected at birth, become spontaneously diseased subsequently; nor, as already stated, have I ever seen a congenital opacity of the lens spread or increase in after-life; and in this respect the congenital differs from all other forms of cataract, either lenticular or capsular, except that which is the result of ophthalmia neonatorum.\* There are, in many cases, cogent reasons for operating early, such as nystagmus, and total defect of vision, &c.; but the danger of thickening, and opacity of the capsule, does not, so far as I have seen, come under this category.

*Capsular Cataract.*—I have never seen the capsule so completely opaque at birth, as to obscure the view of the lens beyond; neither have I ever seen, through a transparent lens, an opacity on the posterior capsule. The most frequent form of this disease is a small white speck in the middle of the anterior capsule, and generally known as *Cataracta Centralis*. As already stated, it may exist in two forms, with or without lenticular opacity. When seen alone, it is often but the size

\* I think it possible that the greyish-white nucleated cataract might, in after-life become both yellowish in colour and hard in substance, although I have no authority for so stating. Besides those cases related at page 65, the only instance presenting such an appearance I ever saw was in Thomas Cartmill, aged 86, where the substance of the opacity was a straw-coloured yellow, with a whitish annulus, and a chalky-white central nucleus; yet it is possible that these may have been the appearances at birth.

of the smallest pin's-head; and when congenital, and not the result of infantile ophthalmia, it almost invariably presents a very regular circular form, with a sharp, well-defined edge. The spot is always of a dull flake-white, but is occasionally surrounded with a greyish-white border,<sup>\*</sup> evidently of thinner material. This annulus is often so very narrow, as only to be observed with a good glass.<sup>\*</sup> When the disease is the result of ophthalmia, this nebular ring is not seen, and the edge of the opacity is generally irregular. Many years ago, I observed that several of these central cataracts were nail-shaped, the head presenting anteriorly, and the tang—sometimes about a line in length, and tapering off to a point—passing into the clear substance of the lens, in which it could be well observed when the eye was viewed in partial profile. Upon examining some modern German publications, I find that this appearance has been denominated posterior pyramidal cataract.

With this species of cataract there is usually a good amount of vision, but varying according to the light; therefore persons so affected are improved by the application of belladonna. The disease has long been recognised by oculists, and was known even in the days of Taylor and St. Yves. Von Ammon is of opinion, that it may be caused by adhesion of the pupillary membrane and anterior wall of the capsule at that point. True congenital central opacity of the capsule is usually double.

*Pyramidal Cataract* is a large central opacity of the capsule, which bulges out beyond the level of the lens, and through the pupil into the anterior chamber. It is always conical, with a circular base, and presents a chalk-white appearance. Fourteen years ago, the late Mr. Hocken, of Manchester, was good enough to forward me a drawing, from which the accompanying illustration was made. Although not very artistic, it affords a good idea of the appearance, viewed in partial profile. Upon the card containing the drawing is the following, in Mr. Hocken's handwriting:—"Congenital Disease:—Conical opaque growth from the crystalline capsule, projecting through the pupil into the anterior chamber." He had promised to afford me the history of the case, when, unhappily for the cause of science, he died. Von Ammon who has paid a good



<sup>\*</sup> By far the best glass I have ever used is the little compound instrument made by Hawes, of Leadenhall-street, London.

deal of attention to the subject, accounts for this disease by a patency of the presumed aperture in the anterior capsule in very early life, through which, he thinks, the substance of the lens bulges. He has also given drawings of the *post-mortem* appearance in an eye affected with this peculiar malformation; but the preparation appears to have been much altered by the spirit when the drawing was made. Wardrop figured and described a case of it, and said that Wenzel related a similar one, but where I have not been able to discover.

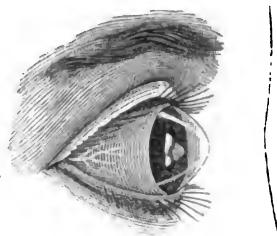
The latest record of pyramidal cataract in this country is that afforded by my assistant, Dr. Wilson, who says:—

“Pyramidal cataract, of which there appear to be two varieties, capsular and capsulo-lenticular, is characterized by an opaque-white cone or pyramid, its base being in or on the lens or its capsule, and its apex projecting into the pupil, and passing beyond the plane of the iris into the anterior chamber. Beer, who was, I believe, the first author to describe this form of cataract, said it was always the product of very acute inflammation of the eye-ball attacking principally the capsule, lens, and iris, and that it was always adherent to the pupil, which was immobile and irregular\*. Beer gives no opinion as to its being congenital, but says it is formed not only by increase in volume of the anterior capsule, but also by a deposition on it of fibro-albumen, which can readily be separated from the capsule; and in this he is borne out by more modern writers, as Mackenzie and Dixon, two of the few English authors who mention this description of cataract. Some writers affirm that it occurs during the progress of ophthalmia neonatorum, when the cornea is swollen, and its posterior surface nearly in contact with the lens; this opinion is held also by Sichel, who gives an illustration of it in the 24th plate of his *Iconographie*; he admits, however, never having seen it follow the ophthalmia of infants, or found any opacity, &c., of the cornea.

“M. F., aged 17, a delicate-looking girl, seemingly much younger than she states, is small in stature, being only four and a half feet high; the head is flattened at top, forehead massive in proportion to face; hair dark brown; lashes very long and black; irides differing in colour. On examination, the following appearances in the right eye are seen. The cornea is altered in shape, being conical, and perfectly transparent; iris of a light-brown colour, having a washed-out appearance, rings of iris very faintly marked; the pupil is circular, and dilates and contracts naturally; no adhesions ap-

\* *Lehre von den Augenkrankheiten*, 1817, bd. ii.

parent; it is occupied by an opaque lens, which, viewed in profile, presents the remarkable appearance I have delineated in the accompanying cut. The cataract involves the whole of the lens, and in front assumes, with the opaque capsule, a pyramidal shape, and protrudes into the anterior chamber; it is composed of several masses differing in colour and consistency; the apex of the pyramid is formed of a soft-looking substance of a blue-white colour; there is a sulcus between it and the base, which latter is apparently firmer; a mass of small opaque-white earthy-looking bodies projects from the temporal side of this latter portion. These are the conical bodies mentioned by writers as easily detached; in the present instance, however, they are situated posterior to the plane of the iris. On the nasal side there is what appears to be a membrane extending from the margin of the opaque lens, to which it is attached, towards the ora serrata; this is, undoubtedly, a portion of the ciliary processes.



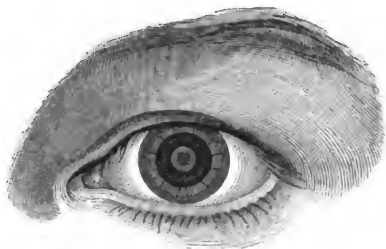
"I could not perceive, on ophthalmoscopic examination, any light reflected from the choroid. The girl has merely perception of light with this eye. There is also present in both eyes a symptom which is generally seen in congenital affections of the eye, nystagmus, or oscillation of the globe, an affection which has been made the subject of a book by Dr. Boehm,\* who has proposed to cure it by cutting the internal rectus; a proceeding which will not, I believe, be undertaken by many practitioners. Conical cornea is, according to Von Ammon, frequently congenital, and often found combined with cataract."

*Capsulo-Lenticular Cataract.*—In most of the cases which I have seen, there was either a central flake, of a dense dull white, and sometimes greyish, or yellowish-drab-coloured, and irregular in form, occupying about a sixth of the central or lower portion of the capsule, and contrasting well with the bluish-white lens behind; in other instances, especially in fluid cataracts, there were several small patches of white upon its anterior surface. In neither of these cases is it possible to discover the precise condition of the posterior segment of the capsule. In the former, that of a single patch of dull greyish-white sub-

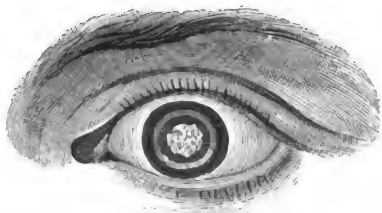
\* *Der Nystagmus und dessen Heilung*, von Dr. L. Boehm, 1857.

† Dublin Hospital Gazette for December, 1860, vol. vii. p. 372.

stance occupying the centre of the anterior capsule, I have found, in operating, that, when touched with the needle, it chipped off like a piece of eggshell; and in one case, that of J. Mooney, here figured, the scale fell into the anterior chamber, and remained there, innocuous, for more than a year, during which time the boy was under my observation. I have observed that there is less regularity between the appearances of the right and left eye, when the capsule is affected, than the crystalline. In the uncomplicated capsular cataract,



the opacity, whether merely central or pyramidal, is always regular both as to shape and position; while in the capsulo-lenticular, such opacity, or opacities, are irregular, both as regards their situation, shape, and colour, and would appear to be more the result of intra-uterine inflammation, than the consequence of any interference with the formative process. In the accompanying illustration of the eyes of Richard Lynn, a boy twelve years of age, made from a drawing taken when the pupils were dilated with belladonna, we have a good representation of one of the most frequent forms of congenital capsulo-lenticular cataract. In the original, the dull flake-white, irregularly-edged, and uneven surface of the large patch of opaque capsule contrasts well with the greyish-white substance of the lens beyond and behind it. In this, as well as other instances that I have met with, the capsular opacity resembled that siliquose condition which follows upon accidents in which, the capsule being ruptured, the substance of the lens is absorbed, and the two hemispheres, thickened and rendered opaque by inflammatory action, adhere, and present the appearance so well known, not merely to the oculist, but to the general hospital surgeon. In Lynn's case, however, the volume of the lens was preserved, although the opaque capsule, viewed in profile, seemed to project beyond its surface; and finally, after the lens was completely absorbed subsequently by keratonyxis, this opaque body, partially obscuring the pupil, had





to be removed, through an opening in the cornea. The needle makes no impression on such formations; if they are small and central, they start from the surface, as in the case of Mooney, already alluded to; but if they occupy so large a portion of the capsule as in the case of Lynn, the cutting side of the needle should be passed round the edge, so as to bring the lens into contact with the aqueous fluid; and, subsequently, if they cannot be displaced from the field of vision, they should be extracted.

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*ART. V.—Two cases of Excision of the Knee-Joint for Forcible Separation of the lower Epiphysis from the Shaft of the Femur.*

By EDWIN CANTON, F.R.C.S., Surgeon to the Charing Cross Hospital, and Lecturer on Surgical Anatomy.

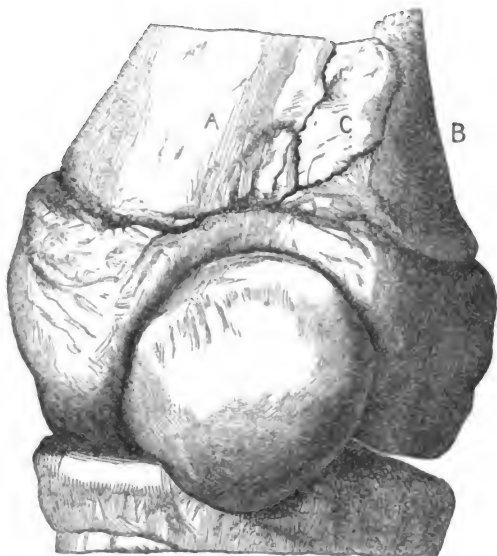
CASE I.—William Jarvis, aged 15, was admitted into the Charing Cross Hospital, under my care, October 21, 1859. At the time of the accident he had sustained, he was playing with another lad, and, with the view of eluding him, was about to run under the body of a horse that stood close by. The animal, however, seeing him approach, suddenly rose on his forefeet, knocked him down, and then kicked him violently, just above the left knee, with one of his hind hoofs. On attempting to rise, the boy found himself unable to stand; and, to escape further injury, he was forced to roll quickly along the ground out of the horse's reach.

On admission, the affected limb was found to be shorter than its fellow; foot quite everted; leg slightly flexed; patella directed outwards; great and general swelling around the knee, and with such distortion of the parts, as to give the impression of the tibia being dislocated backwards, and somewhat outwards. The inner femoral condyle appeared to project unduly, and the skin covering it was tense and abraded. On the outer side, and above the patella, a forward elevation of bone could be felt. By extension and counter-extension, the due length of the limb was restored, the patella resumed its natural position, and the projections referred to became obliterated. The usual appliances maintained adjustment.

No untoward symptom occurred until the end of October, when the incessant restlessness of the patient, from his intolerance of the necessary restraint, gave rise to violent inflammation of the joint. On the inner side of the knee the soft parts

began to slough, and in two days afterwards, the subjacent bone projected through the mortifying tissues. The part protruded was the lower end of the shaft of the femur. The high constitutional disturbance which was set up forbade further efforts to save the limb entire, and I excised the knee-joint, by the usual form of operation, on the 3rd of November.

No peculiarity marked the further progress of the case; and after the lapse of several months, the patient walked out of the hospital, cured.

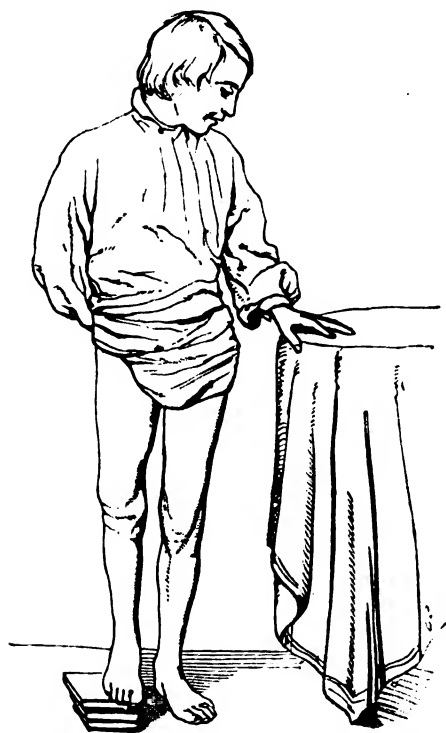


The parts removed show a separation of the shaft of the femur from its lower epiphysis to have occurred in about three-fourths of its circumference (A), while the remaining fourth is still firmly fixed in normal position (B). An oblique fracture extends through the whole thickness of the shaft, and, commencing at its outer side, reaches down to the epiphysis, where it runs into the line of separation between these two parts.

Between the attached (B) and separated (A) portions, a fragment (C) is seen, which, in the engraving, is shown restored to its place; but which had been driven backwards at the time

of the accident, between B and A, completely into the popliteal space, whence I removed it during the operation.

Twelve months after the excision of the joint, I again saw and examined the patient (Nov. 3, 1860). From a photograph then taken of him, the annexed outline has been accurately made.



The tibia and femur were firmly incorporated with one another; the patient experienced no inconvenience, and had walked twelve miles without fatigue.

CASE II.—G. C., aged 8 years, was admitted into the Charing Cross Hospital, under my care, having fallen from the back of a cart, where he had been riding; and, in the descent,

his left knee became entangled between the spokes of one of the wheels.

The joint was found to be greatly misshapen, and an external dislocation of the femur was believed to have occurred; so palpable, indeed, did the nature of the injury appear to be, that the house-surgeon at once, and with ease, restored the form of the part by pressure made in two opposite directions, viz., inwards against the femur, and outwards against the tibia. In addition to this injury, there was a large scalp-wound at the back of the head, which had implicated the branches of both occipital arteries; the hemorrhage had been profuse.

The night was passed in great restlessness, though an opiate was administered. The next day, severe pain in the knee was complained of, and which arose from malposition of the fragments consequent on the patient's inquietude. In effecting re-adjustment, the soft parts against which the lower end of the femoral shaft had been driven were seen to be much ecchymosed; and in a few days, a slough having formed there, and begun to separate, the bone protruded, showing at its lower surface that appearance which characterizes the part for attachment to the articular extremity. Various means were had recourse to, with the view of keeping the parts properly *set*; but, in consequence of the severe nature and painful state of the scalp-wound, the boy, besides his general restless state, was continually throwing his body towards the opposite side of the injury, so that his head might recline sideways on the pillow; hence, the femur was proportionately directed outwards, protruding through, and irritating the wound by which it had so lately found egress. Suppuration soon took place, and threatened to become profuse; the constitutional irritation ran high; the appetite had failed; sleep could hardly be procured; and it speedily became evident that, unless some decisive operative measure was instituted, the patient would die.

I excised the knee-joint, making the H incision; the outer cut included in it the opening already described. After the joint-ends of the femur and tibia, together with the patella, had been removed, a very thin slice was sawn from the free end of the thigh-bone, so that more even adaptation with the tibia might be ensured. No attempt at primary union followed. The restlessness of the patient continued almost unabated, and defied every means adopted to quiet the system, and maintain the limb at rest. (Much of the scalp-wound was still unclosed,

and appeared to be the source of irritation.) About an inch of the femur protruded, and could in no manner be kept reduced and covered; and, it becoming evident that this part tended greatly to increase the general *malaise*, at the same time that it was quickly passing into a necrosed condition, I removed it by the saw. The edges of the wound could be now brought once more together, and were so maintained, with an opening preserved at the outer side, so as to allow of free escape for the discharge, which slowly began to assume a more healthy character.

From this time, matters appeared to progress favourably; the patient became calm; sleep and appetite were restored; the wound of scalp and knee, in process of time, healed. No osseous union, however, took place between the femur and tibia—a fibrous one only resulted. To have cut down again to the bones, and removed more of them, considering what I had had to take away, in addition to the loss entailed by the first operation, would have conferred, in its great diminution of length, as useless a limb as the patient now laboured under, and I had no alternative but to amputate at the thigh. Accordingly, I removed the limb, when the boy's health had become thoroughly re-established. He is living, and well.

*Observations.*—The form of accident, of which the above cases are well-marked examples (*though not even alluded to by many of the highest authorities of the present day*), is, nevertheless, one fraught with great pathological and surgical interest. Valuable illustrations may be obtained therefrom of that improvement in modern surgery, which teaches the preservation of a limb through sacrifice of a joint,—affording, at the same time, a strong contrast to the unnecessary mutilation which formerly characterized the treatment of this injury, or that non-interference which resulted in death.

In an example of this injury, occurring in the practice of Coural<sup>a</sup>, amputation was resorted to. The age of the patient was eleven years. Removal of the limb was also adopted in a similar case related by Adams, of Dublin<sup>b</sup>.

Sir C. Bell<sup>c</sup> relates the case of a lad, aged 13, who met with this accident. “Suppuration took place in the joint; and by the delirious restlessness of the patient, and ulceration

<sup>a</sup> Archiv. Génér. de Méd., tom. ix., 8eme Serie, p. 267. 1825.

<sup>b</sup> Cyclopaedia of Anatomy and Physiology, Art. “Abnormal Conditions of the Knee-Joint,” vol. iii. p. 69.

<sup>c</sup> Observations on Injuries of the Spine and of the Thigh-bone, page 42. London: 1824.

of the integuments, the bone was thrust through. A consultation was necessary; and, on examination, it was found that not a fracture had occurred; but the spongy extremity of the shaft projected, whilst the epiphysis retained its connexion with the joint. The lad died."

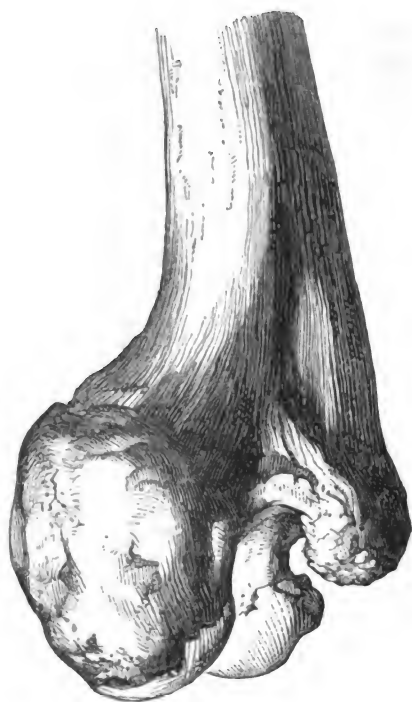


Liston\* has recorded an instance of forcible separation of the lower femoral epiphysis, occurring in a girl, aged fourteen years. Firm union took place very nearly in the same manner as shown in the following case. The knee continued painful and swollen; and there was a halt in walking. After a lapse of about three years, extensive suppuration took place in the lower part of the thigh, and round the knee-joint; and amputation very soon became indispensable for the preservation of life.

Where union of the fragments has eventually taken place, the mischief has not, in certain instances, terminated with their

\* *Elements of Surgery*, Second Edition, p. 721, London: 1840

juncture:—e. g. Sir C. Bell<sup>a</sup> describes a case where osseous consolidation occurred; but the limb was obliged to be removed many years afterwards, on account of a popliteal aneurism occasioned by the projecting extremity of the shaft.



Not only may the epiphysis of a bone be separated from its shaft by violence, but it may also become disunited and displaced from disease. An interesting specimen was exhibited at the Pathological Society of Dublin<sup>b</sup>, of a separated upper

<sup>a</sup> *Loc. cit.* p. 42.

<sup>b</sup> Dublin Journal of Medical Science, 1839. I am indebted to my colleague, Mr. Barwell, for the subjoined reference, wherein, I believe, we may find the earliest recorded instance of the surgical removal of the head of the femur.—“*Coxæ articuli suppuratio cum secessione coxæ femoris solidata.* Anno 1730. Puella rustica, ætat. 14, annorum coxæ articulus tumescit dolet perumpetur chirurgus dilatat foramen naturâ factum, extrahit totum ossis femoris caput. Subjecti posthâc in ulceris cavitatum, *Myrrhæ tincturam porro fluxum Ung. fel. W.*, stringat eam denique arcto vinculo raro deligat, atque sex septimanarum curriculo consolidat, ut puellam postmodum libere liceat manea inseperit.” John Daniel Schlishting, M. D. Philosophical Transactions, vol. xlii. p. 274.

epiphysis of the femur, which was spontaneously extruded, in a case of morbus coxarius. Richel<sup>a</sup> describes the particulars of a case, and adds an engraving of the parts, where the lower epiphysis of the thigh-bone, having become detached in the progress of disease of the knee-joint, was completely displaced to the lower and outer side of the shaft of the bone.

It is, of course, only in comparatively young subjects that the form of injury under consideration can occur. "The several epiphyses of the femur," observes Humphrey<sup>b</sup>, "are united to the shaft in the inverse order of their appearance: the lesser trochanter, soon after puberty; the great trochanter, at about 17; the head, at 18; and the lower epiphysis, from 20 to 25."

In neither of the cases which fell under my own care, had I the opportunity of seeing the patient immediately on his admission; both were received and attended to by my house-surgeon; and I found the limb, in each instance, arranged in splints at my next morning's visit, when I was informed of the mode of production of the injury, and told that a dislocation of the knee had been the result. Reduction was stated to have been accomplished with facility. Judging from the ages of my patients (15 and 8 years), and considering the particular manner in which violence had been inflicted, I expressed my belief that an error in diagnosis had, not improbably, been made; for, such a degree and form of injury would, with the epiphysis still unconsolidated with the shaft of the bone, more readily produce disjunction of the former, than cause a luxation by rending asunder those numerous and strong ligamentous ties, within and without, which complete and fortify so large and complex a joint as that of the knee<sup>c</sup>. My opinion, never-

<sup>a</sup> Diss. de Epiphy. ab ossium diaphysal diductione, in Sandifort's Thesaurus. Roterd. 1768. Klose has seen, in the space of eight years, thirteen cases of separation of the epiphysis from the shaft, as the result of disease, occurring in young subjects; seven of these were of the femur at the knee. He names the affection, separation of the epiphyses (epiphysentrennung), or, meningo-osteo-phlebitis. Prag Vierteljahrsschrift, for January, 1855, p. 97. Stromeyer refers to a similar disease, under the name of ostitis articularis peracuta, or, Arthrophlogosis totalis—*Handbuch der Chirurgie*, vol. i. p. 477.

<sup>b</sup> A Treatise on the Human Skeleton, p. 477,—London: 1858,—a work I cannot too highly commend. "The femur is complete, by the union of its parts, after the twentieth year."—Quain and Sharpey, Elements of Anatomy, Fifth Edition, p. 160. London: 1848. Otto remarks, "I have observed in some men of twenty-two, and in one of twenty-three years, the separation of all the epiphyses; and the same also in the skeleton of a man of twenty-seven years, in the hospital of St. Francis, at Naples."—*Compendium of Human and Comparative Pathology*, translated by South, note 5, p. 127. London: 1831.

<sup>c</sup> "All dislocations of the knee are exceedingly rare, yet we occasionally read of them; and, perhaps, in the course of twenty years, there may be one case brought into a large hospital."—S. Cooper, First Lines of Surgery, Seventh Edition, p. 716. London: 1840.



theless, was, at the time, deemed of little weight, inasmuch as I had not been present to examine either of the patients on his admission; and the symptoms of dislocation had appeared to be so obvious, that the supposed reduction was, at once, and with ease, effected. The issue of the cases, however, sufficiently justified the view I had ventured to entertain, and which had its origin in the above considerations.

In regard to the upper epiphysis of the femur, Roche and Sanson<sup>a</sup> have remarked:—"Chez les jeunes sujets, les causes capables de produire une fracture de col du fémur ne déterminent qu'un décollement de l'épiphyse."

With respect to the mode of production of the injury, in three instances (one of Sir C. Bell, that of Liston, and Case II.), the leg, or knee, became entangled and twisted between the spokes of a wheel in motion. In two other cases (by Sir C. Bell, and Coural), the limb was fixed to above the knee-joint; whilst the body was thrown forwards, or turned round. In Case I. violence was immediately applied just above the articulation; and in Adams' case, as well as in one figured by MacLise<sup>b</sup>, the manner in which the force was directed is not stated.

It has been shown that union by bone may take place after this injury; nevertheless, in two instances where this was the result, subsequent disease, and which might be said to date from the original mischief, necessitated, eventually, the removal of the limb. In the other cases already mentioned, amputation was more speedily required, in consequence of the violent articular inflammation which sprang up with profuse suppuration, to endanger the lives of the patients through the high constitutional disturbance so hastily supervening. Lastly, Case I. has proved with what success excision of the knee-joint may supersede removal of the limb for this accident; and I believe it is not unreasonable to presume that in Case II., the result would have been equally favourable, had the reparative powers of the patient been more adequate to the work of repair; and great, indeed, is the amount of constitutional vigour which the surgeon has to expect before union and consolidation of all the textures he severs can be fully accomplished. In place of a series of destructive processes rapidly advancing, and from a joint radiating, as it were, with still increasing in-

<sup>a</sup> Nouveaux Elém. de Path. Méd-Chirurg., Third Edition, vol. ii. p. 191. Bruxelles. 1834.

<sup>b</sup> On Fractures and Dislocations, Plate xxxi. Fig. 5. The separation of the epiphysis is here accompanied by a transverse fracture of the shaft of the femur in its lower-third.

tensity to the implication of the whole system, the surgeon, by his performance of excision substitutes, so to speak, a condition the equivalent of a severe *compound fracture*; and in bringing the divided parts at once together, follows out a good old surgical maxim, which inculcates the endeavour to render that fracture, as speedily as possible, a *simple* one; when, by attention to position, maintenance of quietude, and removal of discharge, he has *now*\* every reason to anticipate that his measures may not only preserve a threatened life, but retain, also, a useful limb.

ART. VI.—*The Writ "de Lunatico Inquirendo" in the Case of Jonathan Swift, D. D., Dean of St. Patrick's; with Observations.* By J. T. BANKS, M. D., M. R. I. A., King's Professor of the Practice of Medicine; Visiting Physician to the Richmond Lunatic Asylum, &c. &c.

CHANCE lately placed in my hands the legal document which proves that, after due inquiry before commissioners appointed under the great seal, Jonathan Swift was declared to be "a person of unsound mind and memory," and placed under the protection of the Court of Chancery. The original, with the autographs of the commissioners and the jury, I had the honour to bring under the notice of the Royal Irish Academy, at a recent meeting of that learned body.

So much of doubt and uncertainty rests upon many passages in the life of Swift, that it is of great importance to add any fact, the authenticity of which is beyond all question. As a proof of the contradictory nature of some of the statements, I may mention that the mode of his death is still an unsettled point.

Lord Orrery says, "His death was easy, without the least pang or convulsion; even the rattling in his throat was scarce sufficient to give any alarm to his attendants, till some very little time before he expired." Falkner, on the contrary, asserts that "he died in very great agony; having been in strong convulsive fits for 36 hours before." Every circumstance, every event, in the life of the illustrious Dean of St. Patrick's must be fraught with interest, connected as they are with one who

\* "Of 184 cases in all, collected by Butcher and Price, the mortality amounts to 21·2 per cent., which is less than amputation of the thigh; and, in nearly 50 per cent., the result is said to have been a useful limb."—Pirrie, *The Principles and Practice of Surgery*, Second Edition, p. 786. London: 1860.

was not only distinguished by the excellence of his genius, but also by the unswerving character of his patriotism.

To all who are familiar with the history of the life and labours of Swift, it is well known that many years of his life were clouded by the threatenings of impending disease ; and that he himself was painfully conscious that the symptoms under which he laboured were frequently the precursors of mental aberration. The psychological physician will easily recognise, by a study of Swift's eventful history, causes which his experience informs him are potent in the production of mental alienation. The storms to which he was exposed were well calculated to prostrate a fabric which rests on so insecure and unstable a basis as the human mind. Poverty and dependence in youth, so galling to a haughty spirit such as his, disappointed hopes, blighted affections, the loss of those most cherished, with many other circumstances, combined to produce the disease, the anticipation of which had so long, like an evil spirit, haunted him, and well nigh embittered every moment of his existence.

That cerebral disease existed, and can be traced back to an early period of his life, is beyond all doubt. Few and meagre as the records are which we possess on the subject of Swift's disease, and solely derived from himself, and others even less likely to be well informed, we can have no difficulty in arriving at this conclusion.

It does not appear, as far as we have the means of ascertaining, that he inherited any predisposition to psychical disease ; but we are not in possession of any accurate information regarding the mental health of his progenitors.

We know that his grandfather, who distinguished himself by his zeal and sufferings in the Royal cause during the " great rebellion," died at a good old age ; and we do not hear of his having been afflicted with any mental infirmity. His father died young, before his son—who was destined to play so conspicuous a part in the busy world of politics and literature—had seen the light. It may be supposed that from his mother Swift inherited much of that extraordinary humour for which he was so remarkable. If this be so, as there is reason to believe, it is only in accordance with a fact often noticed, namely, that talent is more frequently transmitted from the mother than the father.

That the offspring not only resembles the parent in outward form and feature, but also frequently in mental endowments, we have ample proof from daily observation. No fact is more familiar to the physician, than the transmission of disease, psychical as well as physical ; and the same may be affirmed

with regard to mental peculiarities. This, I believe, may be looked upon as a fact ascertained by reiterated observation; nevertheless, Mr Buckle, in his *History of Civilization*, has attempted to shake our well-founded faith in the transmission of hereditary talents, hereditary vices, and even of "hereditary madness." If the distinguished and eloquent author had taken the trouble of consulting the records of hospitals for the insane, or the works on mental disease, he would find himself compelled to give up his idea regarding the non-hereditary nature of mental maladies.

M. Moreau, Physician to the Bicêtre, one of the most accurate observers with whom I have ever come in contact, strongly advocates the doctrine of the transmission of psychical disease in his learned and deeply interesting work, "*La Psychologie morbide dans ses Rapports avec la Philosophie de l'Histoire ou de l'Influence des Neuropathies sur le Dynamisme Intellectuel.*" He says, "*L'hérédité est la source des neuf-dixièmes, peut-être, des maladies mentales.*" In this opinion of the hereditary nature of insanity, he only agrees with all who have studied the subject by the light of experience—Esquirol, and other French alienist physicians; and the most distinguished English psychologists, as Conolly, Bucknill, Winslow, and others.

Swift is included by Moreau in his group of men who presented symptoms of "insanity, properly so called;" and in goodly company, doubtless, is he placed; but probably not one among these aristocrats of talent was possessed of a more original mind. M. Moreau considers that diseased states of the brain powerfully favour the development of the highest intellectual functions: it would appear, according to him, that in point of fact the most exalted genius and insanity are cognate conditions. He evidently agrees in opinion with the ancient sage who says, "*Nullum magnum ingenium sine mixtura dementiae.*" Swift himself said, "Madness is the source of all human genius."

The subject is one of surpassing interest; and it must be conceded to M. Moreau, even by those who differ widely from his conclusions, that he has handled it with much ingenuity and ability.

It is not my present purpose to enter minutely into the subject of Swift's case, from a medical point of view; that has already been done by my friend Dr. Wilde, in the pages of this Journal, and well has he performed the difficult task which he assigned to himself, and that too in the absence of any record from the many physicians who ministered to the Dean.

When we remember how eminent some of the physicians

were who endeavoured to alleviate Swift's sufferings, it is a subject of much regret that they have not bequeathed to posterity one line on the subject of the disease of their illustrious patient.

Among his physicians were Sir Patrick Dun—the benevolent founder, like himself, of an hospital which bears his honoured name—and Dr. Arbuthnot, physician to Queen Anne, of both of whom he speaks in terms of affection. Of Arbuthnot he says, "He was the only man of the faculty who seemed to understand my case; but could not remedy it." Writing to Pope, he says, "The death of the Doctor (Arbuthnot) has been a terrible wound to my heart." Swift had begun to feel acutely the loss of friends, which Sir William Temple said is a "tax on long life."

Numerous as the Biographers of Swift have been, and carefully as they have sought for information from every available source, there is one passage in his life, and that the most melancholy, with which they appear to be unacquainted,—I allude to the fact of his having been made a "Chancery Lunatic." It is true, Sir Walter Scott alludes to persons having been appointed to take care of him. He says, "His estate was put under the management of trustees, and his person confided to the care of Dr. Lyons, a respectable clergyman."

Dr. Delany says, "In the beginning of the year 1741, his understanding was so much impaired, and his passion so greatly increased, that he was utterly incapable of conversation; strangers were not permitted to approach him, and his friends found it necessary to have guardians appointed of his person and estate." We shall see, by referring to the legal instrument, how incorrect Delany is as to the date. It was not until the 17th of August that the writ "*de lunatico inquirendo*" was issued; and it may be remarked, that it was returned on the 19th of the same month, although the return was not required before the 3rd of November. It is evident, then, that the biographers knew nothing of the writ, or this inaccuracy could not have occurred. It is strange, from this statement of Delany's, and from other proofs of the utter prostration of Swift's intellect, at a period long antecedent, that the jury only found him to be of unsound mind for three months prior to the date of the Commission.

That we have no record of the examination of medical men or "experts," on an inquisition which was to decide upon the mental soundness or unsoundness of a man who "did the highest honour to his country by his parts, and was a great blessing to it by the vigilance and activity of his public spirit," is matter of regret. An inquisition upon such a man

must have excited much interest, and the more strange that all the facts connected with it should not be a matter of public notoriety. But we do not find that even the long and laborious research of Mr. Weld Hartstonge obtained for his friend, Sir Walter Scott, any information on this point. It appears, also, that Scott obtained materials for his life of Swift from the Rev. Mr. Berwick, "so well known to the literary world," and from other sources likely to be in possession of authentic information.

I may mention, that Judge Berwick, son of the Rev. Mr. Berwick, to whom Sir Walter Scott expresses his obligations, is the possessor of a portrait of the Dean, which is probably the best which has ever been painted.

The petitioners for the writ de lunatico inquirendo were the Rev. John Grattan and the Rev. James King, both of whom were executors of the will, and to each the Dean made a bequest.

"I bequeath to Mr. John Grattan, Prebendary of Clonmethan, my silver box in which the freedom of the City of Cork was presented; in which I desire the said John to keep the tobacco he usually cheweth, called pigtail." "I bequeath to the Rev. Mr. James King, Prebendary of Tipper, my large gilded medal of King Charles the First, and on the reverse a crown of martyrdom, with other devices."

Of the Commissioners to whom the writ was directed, the first on the list is the Right Hon. Luke Gardiner.

Dr. William Cooper was Chief Examiner of the Court of Chancery.

Charles Grattan is marked on the list of Swift's friends with the letter G. All his friends were classed into grateful, ungrateful, indifferent, and doubtful.

Alderman Perceval Hunt was Sheriff of Dublin in 1718, and Lord Mayor in 1755. Alexander M'Auley was one of the executors of the will; he was Judge of the Consistory Court, and one of the first Governors of Swift's Hospital. By the will we learn that M'Auley was one of Swift's most esteemed friends. He is thus mentioned:—"To Alexander M'Auley I bequeath the gold box in which the freedom of the City of Dublin was presented to me, as a testimony of the esteem and love I have for him, on account of his great learning, fine natural parts, unaffected piety and benevolence, and his truly honourable zeal in defence of the legal rights of the clergy in opposition to their unprovoked oppressors."

Alderman Macarel was Lord Mayor in 1738, and Alderman Pearson was Lord Mayor, 1730.

John Rochfort was one of the witnesses to the will.

Sir James Somerville was Sheriff in 1720, Lord Mayor in 1736, and at the same period was knighted. This gentleman was subsequently created a Baronet, by patent, dated June 14, 1748.

Eaton Stannard was Prime Sergeant at Law, and Recorder of Dublin; he was one of the executors of the will.

Philip Tisdall, Judge of the Prerogative Court; Whitney Boleyn, King's Counsel.

Some of the Commissioners, it may be noticed, were Swift's intimate friends, from the circumstance of his having selected them as executors to his will, or having remembered them in that document.

The writ was directed to seventeen gentlemen named, as Commissioners; but only seven names are annexed to the inquisition. The writ commanded any three or more of them to diligently inquire into the case.

The jury, upon which devolved such an important duty as determining the mental condition of Jonathan Swift, was strangely constituted. There are some respectable citizens, and probably, from their position in life, intelligent men; but there are also two carpenters and a currier associated with them. Their task, however, was an easy one, under the guidance of the Commissioners; for, from all we can learn, we may fairly come to the conclusion, that long before the time of the issuing of the writ, the mighty mind of Swift had completely broken down. The first faint clouds which foreshadowed the coming event may easily be traced to a comparatively early period in the life of this truly great man; but the storm which was destined to utterly overwhelm the shattered bark, had already broken over it.

The life of Swift is still to be written; and I have reason to believe it is in progress, and, moreover, that it is in good hands. My object in publishing the contents of the important instrument which I had the good fortune to discover, is to fill up what has hitherto been a blank in the darkest page of the history of the renowned Dean of St. Patrick's.

"George the Second, by the grace of God, of Great Britain, France, and Ireland, King, Defender of the Faith, and so forth, to our trusty and well-beloved the Right Hon. Luke Gardner, Esq.; Eaton Stannard, Recorder of the City of Dublin, Esq.; Philip Tisdall, and Boleyn Whitney, Esqrs.; Doctor William Cooper, and Doctor Thomas Trotter; Sir James Somerville, Alderman John Macarell, Alderman Percival Hunt, Alderman Nathaniel Pearson, Alderman Robert

King, Thomas Le Hunt, and Alexander M'Auley, Esq., William Harward and John Rochfort, Esqrs., Charles Grattan and Bellingham Boyle, Esqrs., greeting: Whereas it is given us to understand, by the petition of the Rev. John Grattan and the Rev. James King, that the Rev. Doctor Jonathan Swift, Dean of St. Patrick's, Dublin, hath, for these nine months past, been gradually failing in his memory and understanding, and of such unsound mind and memory that he is incapable of transacting any business, or managing, conducting, or taking care either of his estate or person: We, being willing to provide a remedy in this behalf, do command you, or any three or more of you, that you repair to the said Doctor Jonathan Swift, and, by all proper ways and means, you examine him; and, moreover, by the oaths of good and lawful men, by whom the truth of the matter may be best known, you diligently inquire whether the said Doctor Jonathan Swift be a person of unsound mind and memory, and not capable of taking care of his person or fortune, as aforesaid; and if he be, how long he hath been so, and of what lands and tenements, goods and chattles, the said Doctor Jonathan Swift was possessed of at the time he so became of unsound mind and memory, or at any time since, and what is the yearly value thereof, and who is his next heir; and such inquisition as shall be then found, you, or any three or more of you, shall, openly and distinctly, make return thereof to us, in our Chancery in Ireland, on the third day of November next, under your seals and the seals of those by whom the said inquisition shall be made, together with this writ.

"Witness our Justices General and General Gouvernours of our said Kingdom of Ireland, at Dublin, the Twelfth day of August, in the Sixteenth Year of our Reign.

"DOMVILE.

"Exd. Ed. Madden,

*Deputy Clerk of the Crown and Hanaper."*

"The executors of the within commission appear by the inquisition hereunto annexed—

LUKE GARDINER,  
PHILIP TISDALL,  
HENRY ROCHFORD,  
WILLIAM HARWARD,  
BELLINGHAM BOYLE,  
PERCIVAL HUNT,  
JOHN MACARELL.



"An inquisition taken before us, the Right Hon. Luke Gardiner, Philip Tisdall, Esq.; John Macarell, Percival Hunt, Alderman William Harward, John Rochfort, and Bellingham Boyle, Esqrs., Commissioners by virtue of a Commission of our Sovereign Lord George the Second of Great Britain, France, and Ireland, King, Defender of the Faith, and soforth, Bearing Date the Twelfth Day of August, in the Sixteenth year of the Reign of his said Majesty, at the Deanery House of St. Patrick's, Dublin, the seventeenth day of August, in the year of our Lord one thousand seven hundred and forty-two.

"The Names of the Jury of honest and lawfull Men sworn to enquire and examin of and into the matter specified in said Commission.

EDWD. HUNT, Alderm<sup>n</sup>.

JOHN ADAMSON, Merch<sup>t</sup>.

ROBT. DONOVAN, Merch<sup>t</sup>.

ARTHUR LAMPREY, Chand<sup>r</sup>.

THOMS. HAMILTON, Brewer.

JOHN WALSH, Carpenter.

JOHN COOKE, Hosier.

JOHN CUMIN, Carpenter.

ERASMUS COPE, Jeweler.

JOHN SICAN, Merch<sup>t</sup>.

JOHN MARTIN, Currier.

JOSHUA BARRINGTON, Merch<sup>t</sup>.

"Gentlemen,

"Your Issue is to try and Inquire Whether the Rev<sup>d</sup> Doctor Jonathan Swift in the annexed Commission named be a person of unsound mind and memory, and not capable of takeing care of his person or Fortune; and if he be, how long he hath been so, and of what Lands, Tenem<sup>t</sup>, goods, and Chattles the said Doctor Jonathan Swift was possessed off at the time he so became of unsound mind and memory, or at any time since; and what is the yearly value thereof, and who is his next Heir."

"Wee Find that the Rev<sup>d</sup> Doctor Jonathan Swift in the annexed Commission named is a person of unsound mind and memory, and not capable of takeing care of his person or Fortune, and that he hath been soe since the twentyeth day of May last past. And Wee Further find that the said Jonathan Swift was on the said twentyeth day of May, and still is seized and possessed of Lands, Thytes, and Tenements of the clear yearly value of eight hundred pounds sterling, and also possessed of goods and Chattles to the value of ten thousand pounds sterling; and It does not appear to us who is his next Heir."

(Here follow the signatures and seals of the twelve "honest and lawfull men" whose names have been recited.)

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*A Book about Doctors.* By J. CORDY JEAFFRESON, Author of "Novels and Novelists," &c., in two volumes. London, Hurst & Blackett. 1860. 8vo, pp. 628.

"A Book about Doctors." What a grand theme! ennobling to the very writer thereof, the history of philanthropy, benevolence, and natural science—the history of the great and good men who, in all times and climes, have alleviated human suffering, cheered the desponding, comforted the afflicted, poured out the oil and wine on the way-worn and the wounded; the story of the brave and gentle; of those whom the Scripture desires us to honour, who have more than all others carried to their fellows the message of Christianity—"Peace on earth, good will towards men;" and who can, above all others, be tried by that great test, "I was sick, and ye visited me"—the record of the men and the art which has brought strength to the weak, sight to the blind, ease to the tortured, sleep to the weary, and sense to the demoniac—many of whom have died for their brethren, and several of whom have bequeathed their fortunes to the poor. Verily, a glorious subject; let us see how it has been handled by Mr. J. Cordy Jeaffreson. Here is "The Book about Doctors," in two volumes, octavo, bound in gold and morone, with a frontispiece to the first, copied from Hogarth's caricature of the old broad-wigged doctors, smelling their vinaigrette-canes, and tasting the contents of a male urinal! And who are the doctors? Mesmer, the impostor; Roger Grant, the tinker; Valentine Greatrakes, the stroking doctor, whose "cures" we gave an account of in vol. iv. p. 254, of this Journal; Louthburgh and his wife, who humbugged the public into the belief that *their* faith had made them whole; Townsend; St. John Long; Morrison, the pill manufacturer; Graham; and a host of other equally notorious quacks, from the days of Merry Andrew, and Katterfelto the

conjurer and mountebank, down to the present time. If the work had been labelled "A Book about Quacks, by a Humbug," the title would not have been inappropriate. And yet this is a book which a London weekly contemporary styles "a compliment to the medical profession, and an acquisition to its members; a book to be read and re-read; fit for the study and the consulting-room, as well as the drawing-room table and the circulating library." No wonder that another London contemporary, celebrated for its sneers at medicine, should say, on reviewing such a work as Mr. Jeaffreson's, "There is no denying the fact that, though medicine does hold its place as one of the *learned* professions, it does not do so without a struggle." And again, "The ministering to the wants and diseases of the body is by itself a gross and menial office." England is *par excellence* the land of quacks, and therefore we can understand why a gentleman of "the Rolls Chambers, Chancery Lane," who has so many opportunities of studying the character of the quack and mountebank, should be an authority upon such subjects as this so-called "Book about Doctors" treats of.

To make up and make out a book about doctors, we have a few biographies of well-known celebrities, such as Radcliffe, Mead, Arbuthnot, Blackmore, Cooper, Abernethy, &c., injudiciously abridged from well-known sources, interlarded with a few hackneyed professional jokes, and stale pointless anecdotes, so tamely told, that one is inclined to think the author does not see the wit of them himself. A good example of this is given in the recital of Curran's interview with Abernethy, in which Mr. Jeaffreson makes the ex-Master of the Rolls introduce himself to his countryman, by saying, "My name is John Philpot Curran," whereas the gist of the story is that Abernethy made a diagnosis of the man before the latter had time to announce his name. Will our readers forgive us, if we relate the anecdote as we had it more than once, twenty years ago, from the lips of dear old Cliff, in the Museum of the London College of Surgeons :—

When Curran left Ireland, and took up his residence in London, before the days of Clubs and Athenæums, he suffered from *ennui* and dyspepsia, the natural consequence of want of friends and occupation, but more particularly of that choice and witty society of which he himself was the ornament, from the time of the 'Monks of the Screw' to the last days of his residence at Rathfarnham. Like all the dyspeptic world of the day, he sought the aid of Abernethy, and, lawyer-like, wished to state his case; but as such harangues were not permitted in

Abernethy's summary court of medicine, the great orator was shut up with the brusque "Yes, I know; it is all your stomach. Here's a prescription; take one of these blue pills; eat my biscuits; read and attend to the directions in this little book; and call the day after to-morrow." In vain the lawyer essayed to speak; he was marched to the door, where another patient was in readiness to occupy his place.

The scene was repeated in a few days; the doctor looked at the patient's tongue, and recommended a continuation of the treatment. Upon the third visit (for story-mongers, as well as fairy-tale relaters, must have a climax, and three is a mystical number), Abernethy was writing a letter, when the patient was shown in; and, startled by hearing the door-key turned in the lock, he saw, on looking up, the yellow, monkey-faced, but bright-eyed Irishman, whose case he would not listen to, deliberately putting it in his pocket. His first impression was that the man was a lunatic, and so he measured his patient, and looked to the poker. On the first exclamation, Curran, who was a most consummate mimic, assuming the tone and look of Abernethy, said, "Sit down; show me your tongue; it's all your stomach; I know all about it." So astonished was the great physician, that he motioned his eccentric patient to a seat, who, resuming his natural voice, said, "Now, sir, I have been here twice, paid you your fees, followed your directions, taken your pills, eaten your biscuits, and read your trashy book; and I am not one bit bitter; and the reason is, that you have never listened to the history of my symptoms." On this, Abernethy lay back in his chair, folded his hands resignedly, and said, "Well, sir, go on; begin at the beginning; and give an account of your mother and grandmother, and relate the family history of your whole seed, breed, parentage, and education." The patient, assuming the attitude, and imitating the nasal twang of the street-criers of last speeches and dying declarations, drawled out, "I was born of poor, but honest parents, in the town of Ballyragget;" and thus proceeded, until he arrived at, "and my name is——," when Abernethy, starting up, exclaimed, "John Philpot Curran!" "Yes," said the patient. "Then, why the devil did you not tell me so at once? You are not a bit sick, my dear Curran; don't take any more physic, but come and dine with me to-day." Cliff told the writer of this that he was present at the dinner, and saw the scene rehearsed by the pair of worthies. Abernethy and Curran became, as it is known, fast friends.

The anecdote has a moral: the patient must be heard; and

the less real disease, the more graphic and lengthened the details. It must be a great comfort to a patient to unburthen himself of the pent-up imaginings of his disease, which he has rehearsed again and again before he came to get a guinea's worth of listening, and a prescription. We remember a fellow-student at a German university, who, we were happy to hear, some years after we had met, was rapidly acquiring a lucrative practice. On asking a friend how this good luck had come about, the answer we received was, "He listens, be the story ever so long—he never interrupts—that is the great secret of his success."

Some years ago, an old lady, who had been for some time a patient of the writer's, for a very simple and ostensible disease, waited upon him, and said—"Now, my dear doctor, I know your time is precious, so here are two guineas, and you must listen to what I have to say." The good lady detailed her symptoms, real and imaginary, then appealed to her maid as to whether she had forgotten any, and finally producing a long list from her pocket, repeated her case: and, rising to go, thanked the surgeon for his extreme kindness, and said she had no doubt that she would derive great benefit from his advice, although he had never opened his lips, nor written a line. Some people might think the money hardly earned.

We remember attending an old judge, a most worthy and learned man, but somewhat pragmatical and pedantic. There were four of us in attendance; after assembling in the drawing-room daily, we were ushered in a body to the sick chamber, where there was little light, and the bed-curtains were closely drawn. The patient having been assured of our presence, then opened the pleadings, stated the case as it occurred since the last visit, examined the various symptoms as witnesses, charged the jury, admitted the light, and then demanded our verdict.

Lest we might fall into the story-telling vein, we must get back to the subject of our review. It is true that the author has reprinted the exposition of all the quacks he mentions; but the charlatans are so mixed up with the true men, that the general reader, and the public, must look on all as part of the "fallacies of the faculty;" and, even when making short extracts from Pettigrew, and other writers, it is not always the bright side of the man that is presented to the public palate. Thus we read of the "parsimonious temper and habit" of Sir Hans Sloane; the extreme dirt and slovenliness of Humphrey Davy; the rudeness of John Hunter; and the drunkenness of Friend, of whom, by the way, Mr. Jeaffreson tells the story of

"Dead drunk, by G——," when feeling a titled lady's pulse, —which, in this country, we are in the habit of attributing to Harvey.

A Book about Doctors is much wanting; but not such a work as that under review, which, such as it is, is almost entirely about Englishmen—the Scotch and Irish medical writers are ignored. No—not altogether; the Dublin men are represented, not by Molyneux, Mosse, Dun, or Cheyne, or Graves, or Colles, or Crampton, but by — who, gentle reader, do you think? Brennan—jolly, drunken, satirical, pasquinading, wrestling, turpentine Brennan—one of whose worst poems, that on the "Dublin Doctors," is quoted; but the fun of which cannot be felt, nor the sarcasm understood, by many, even of ourselves, in the present day. But, although the readers of Mr. Jeaffreson's book may not understand all the allusions to Doctor Big Paw, and "sweet Breeny Creepmouse, O," this quotation from the Broadstone slang they must have specially relished:—

"Here ends my song on Doctors, O,  
Who, when all damn'd  
In hell are cramm'd,  
Will beggar all the Proctors, O."

*Diseases of the Heart; their Pathology, Diagnosis, and Treatment.* By W. O. MARKHAM, M. D., F. R. C. P., &c., &c. Second Edition. London: Churchill. 1860. 12mo., pp. 276.

THIS is a second edition of Dr. Markham's treatise, and is, in many respects, a great improvement on the first: much of the introductory matter has been omitted or curtailed, and the whole work has been carefully revised.

The exposition given by Dr. Markham of the symptoms and physical signs of the diseases of the heart is clear, concise, and comprehensive, and renders his work one well adapted as a text-book for the student who is entering on the study of these diseases at the bed-side; and even practitioners, and the readers of the larger books, would often find the short physiological and rational explanations of the principles of diagnosis here given a great assistance. Some books are *hard* for the student, as has been aptly remarked by Dr. Latham, from the very abundance of their matter, and especially if they are at all controversial in their style. To the student such books are often as obstructive, as is the loquacity of many guides and cicerones to tourists. To clinical students, says Dr. Latham, there is no greater im-

pediment of knowledge than over-teaching: the teaching which they most require is suggestive, they have the realities themselves to learn from, the original *book* to read, upon which all sound instruction is but a commentary. Therefore the commentator should only interpose when and where he is needed, and not after the manner of certain critics, who must help us with their annotations, where the meaning of the author is clear beyond dispute. Now, Dr. Markham has avoided the error deprecated by Dr. Latham, and has equally steered clear of the other error of obscure brevity; therefore it is that, though we scarcely find a new fact added to the clinical history of cardiac diseases, we recommend the work as a valuable text-book for clinical study. But, valuable as we believe it capable of being in this respect, we think that to the practitioner who may not have very extended opportunities for acquiring experience in the treatment of these diseases it is likely to be still more valuable, from the application he will here find of the doctrines established by the recent advances of pathologic and therapeutic science. This is the most important feature of the work, and the one in which most novelty will be found.

The whole science of medicine has of late been undergoing a process of fermentation, required, our author says, for its purification,—for the removal of the clogs and burthens of errors and crudities which have gathered around it. When we compare the pathology of to-day with the pathology of the past—the diagnosis of to-day with that of the past—the clinical study of disease, as taught to-day, and taught a generation ago—we need not feel any surprise that the practice of medicine has been absolutely revolutionized. The translator of Skoda, and a disciple in the school of philosophical scepticism, it will not be wondered at that Dr. Markham questions the power, or rather asserts the powerlessness of certain agencies, as, for instance, venesection and mercury, in arresting inflammatory processes; and that, while he denies that disease can be evacuated at the mouth of a vein, and that inflammations can be jugulated or strangled in their birth by venesection, he attempts to show the true use of bleeding and of mercury in treatment. Speaking of the treatment of pericarditis by venesection, advocated by Bouillaud and others, he says:—

“A better pathology and a truer method of observation have forced upon the physician, during the last few years, a conviction of the inutility, and sometimes injurious effects of large venesections upon the progress of internal inflammations; and even they, happily few, who still follow the practice of former days, in the treatment of acute internal inflammations, admit that it is only

during the very early periods of the pericardial inflammation, and in patients of strong and robust constitutions, that venesection is of service. That a moderate bleeding may be practised with impunity, *under such conditions*, there is little doubt; and that it often is of much benefit in giving temporary relief to the sufferings of the patient, and in relieving the congestion of the heart and lungs, is certainly true; but that such a bleeding has *any other beneficial effect, or any direct influence over the progress of the inflammation, has yet to be proved.*

"In considering the question of the value of venesection in pericarditis, we must remember:—That every inflammation is a disease of weakness, a condition which, in reference to health, is an asthenic condition; That heat, redness, and increased vascularity are no more signs of a sthenic circulation, than is the hyperæsthesia of a paralyzed limb a sign of increased vigour of its nerve force; That rheumatic pericarditis occurs exceptionally only in persons of robust constitution; That an organ is involved in this inflammation, the constant performance of whose functions is indispensable to life; And that one of the most immediate effects of the inflammation and its products, is to induce a paralysed condition of the muscular structure of the heart; That in pericarditis, the reaction of depression consequent upon the excitement, is great, and sets in early; and that it is in those cases in which the inflammation appears most violent at the onset, that we are most cautiously to watch for and to expect the greatest amount of subsequent depression; That bleeding, moreover, will not arrest the exudation, but, on the contrary, in certain states of the body, will hasten and increase the amount of it; and that, inasmuch as endocarditis is very commonly associated with the pericarditis, bleeding, by promoting the tendency in the blood to the deposition of its fibrinous particles, will increase the danger of permanent injury to which the valves are exposed, through the deposition of fibrine upon them; And that venesection, as we well know, increases the amount of fibrine in the blood, and diminishes that of the red globules—the increase of the one and the diminution of the other being both indicative of weakness.

"Another special danger has been strongly pointed out by Dr. Todd (*Renal Diseases*, p. 412). He says:—

"An active antiphlogistic treatment creates asthenia; asthenia gives to both rheumatic fever and gout what I may call *the shifting character*, which in both diseases is most perilous. When you find this shifting tendency, depend upon it that the asthenic condition of the patient is that which demands your earliest attention.' 'In this case of acute rheumatism,' he says again at p. 12, 'the loss of large quantities of blood from hæmaturia at an early period of the disease, has not sufficed to keep off a severe attack of pericarditis, . . . nor has it saved the patient from swollen and exquisitely painful joints. On the contrary, the articular, as well as the cardiac symptoms, have been much less tractable than usual.'

"Experience has also shown us, that venesection has no *directly*



beneficial influence over pericarditis; and that large bleedings are prejudicial, and therefore inadmissible in this disease. Nevertheless, that small bleedings are often of very great service *in relieving the congestions of the heart and lungs*, which so often arise as consequences of, and coincidently with the pericarditis, is, I think, an undoubted fact. I have elsewhere spoken of this particular mode of action of venesection; and if the conclusions there arrived at are, as I firmly believe, correct, it necessarily follows—that moderate venesection, practised for the object indicated, viz. to relieve the congestion of the heart and lungs, may be often resorted to with great benefit to the patient.

“In all stages of diseases of the heart in which congestion of the organ occurs, such venesection—duly adapted to the individual case—is of great service. I consider that I have even seen life preserved by timely abstraction of blood in cases of chronic valvular diseases of the heart, where the organ was so overwhelmed and labouring, as to render death imminent.”

The paper read by Dr. Markham, before the Medico-Chirurgical Society, in 1858, at the time of the “blood-letting controversy,” will, probably, be in the recollection of most of our readers. In it he propounded the doctrine here advocated, that the benefit to be derived from venesection, in cases of internal inflammation, arose solely from the relief thereby afforded to the congestion of the heart produced by the disease; and he gave cases illustrating the benefit arising in this way in pneumonia, inflammation of the viscera of the abdomen, &c. The local abstraction of blood, on the other hand, he asserts to be serviceable, by its effects in emptying the vessels of the inflamed part, and, consequently, the practice is stated to be beneficial only where the blood can be taken from a part having a vascular connection with the inflamed organ. In the case of the pericardium and pleura, this is maintained with the walls of the chest by the internal mammary artery, and hence the well-established benefit derived from leeching in pericarditis; but in cases of endocarditis, the application of leeches to the chest is not, Dr. Markham believes, beneficial. In the main we believe these principles to be true; but it is too well established to be doubted, that leeching over inflamed viscera which have no vascular connexion with the surface is often beneficial, on the principle, probably, of revulsion or derivation.

Mercury, in acute pericarditis, our author believes to be only serviceable when used to regulate the secretions. The induction of its specific effects, once considered essential, is now, he says, generally abandoned: Dr. Taylor's observations first demonstrated its inutility, and subsequent experience has

fully confirmed his opinion. The practice never seems to be much adopted on the Continent, and in Germany it is untried and unknown. Gendrin's opinion, that mercury is completely useless in pericarditis, is, Dr. Markham says, the general opinion of his countrymen.

The salts of potash are proved, by experience, to be most beneficial in the treatment of acute rheumatism; and the author confirms the statements of Dr. Garrod, that they not only shorten the duration of the articular affection, but also lessen the liability of the heart to become affected; and when the heart has been affected before the treatment has been commenced, the salts of potash check the progress of the disease, and prevent the terrible mischief which, when uncontrolled, it so frequently produces. The use of opium to relieve the pain, that is sometimes very severe, is also warmly advocated; but care must be taken not to lock up the secretions, and it is recommended to combine it with small doses of compound colocynth pill. Dr. Markham sums up the general treatment of acute pericarditis in the following extract, with which we must close our notice of this valuable treatise:—

“The *general* treatment, then, of rheumatic pericarditis is in the main—at least during its first periods—the treatment of acute rheumatism. In the commencement of the inflammation, the *local abstraction* of blood, proportioned to the degree of pericardial pain and the strength of the patient, is of service. Should there be signs of co-existing pulmonary and cardiac congestions, small venesections should be resorted to for their relief. Warm fomentations or poultices should be constantly applied over the præcordial region—a method of bringing warmth to the inflamed part, and of giving relief to the patient, which is worthy of more attention than is generally given to it in this country. The beneficial effects of blisters have been doubted by some observers; but I have so often seen them followed by mitigation of the local pain, and apparently also by diminution of the pericardial effusion, that I do not hesitate to use them, when the acute periods of the inflammation have passed away. They do not prevent the application of poultices or fomentations. Dr. Todd advises their use, even in the acutest stages of the inflammation.

“The secretions of the body should be duly attended to during the whole course of the pericarditis; for there is, we must consider, a poison to be eliminated from the system. The bowels should be regularly relieved; and the action of the kidneys maintained. The action of the skin, evidently the chief natural eliminator of the rheumatic poison, requires no promotion in this affection. Frequent purging must be avoided; for the partial exposure to cold, which is its necessary consequence, and the extreme pain and suffering which movement is apt to occasion in these cases (when the co-ex-

isting inflammation of the joints is severe), more than counterbalance, by the nervous excitement they produce, the good which the purging might be supposed otherwise to bring about, as a process of elimination.

"In every case of pericarditis, whether rheumatic or non-rheumatic, and at all periods of the inflammation, the condition of the heart's powers should be carefully watched. Light and easily-digestible nourishment—milk and broth—should be administered during the earliest periods of the inflammation; and stimuli, when the acute period has ceased, and the process of absorption has commenced, or whenever the signs of enfeebled circulation begin to show themselves.

"Of the treatment of the secondary disorders which result from the pericarditis through injury done to the heart's structures; of congestions of lungs, abdominal organs, and brain, &c.; I shall speak hereafter, under the head of valvular diseases."

*Further Observations in several parts of Surgery.* By BENJAMIN TRAVERS. To which is appended an Original Memoir or Review of the Nature and Treatment of some Unusual Forms of Eye Disease, by the late Benjamin Travers; dated, 1828. London: Longman, Green, Longman, and Roberts. 1860. 8vo, pp. 205.

THE work whose title heads these remarks consists of two parts; of which part one is the produce of the author's pen, containing observations on "Unusual Forms of Injury of the Hip-joint," "History of a Dislocation of the Left Thigh-Bone into the Sciatic Notch," "Observations upon the Nature and Treatment of Boil and Carbuncle," "Clinical Remarks on Hernia," "A Chapter on Hemorrhage, and the Means to be employed for its Suppression," "A Case of Tetanus," "Diseases of the Breast"—this last apparently being a review of M. Velpeau's celebrated work on this subject; whilst the second part contains a letter from the late B. Travers to Dr. Bauer, on some forms of ophthalmic disease. This letter we shall dispose of by remarking, that, though desultory in its nature, it contains, like all the productions of its distinguished writer, much to interest a profession already under deep obligations to him for some of the most philosophical and practical writings in our medical literature. Our remarks on part one, however, cannot partake of the same general character: it will require a more searching investigation at our hands, not so much, perhaps, with reference to the topics of which the author treats, as to

the incidental remarks made almost, as it were, accidentally by him, in discussing these several subjects already enumerated.

In his introductory remarks, we read as follows:—

*“Objections to Excision or Resection of Joints.*—In the early stage this kind of mutilation is inadmissible. 1st. Because the constitution is unimpaired, and is accessible to the impressions of milder modes of treatment. 2nd. Because a large proportion of cases recover, if carefully and skilfully handled: a partial or even complete ankylosis being the common consequence of such efforts. 3rd. Because a system or habit of body which cannot bear milder measures is in no sense calculated to sustain the shock of so violent a proceeding; to say nothing of the demands subsequently created in view of a tedious healing process. Consequently we find that few of the successful cases survive long, to say nothing of those who sink within ten days or a fortnight after the operation of excision. Whatever may be said to the contrary by interested witnesses, I believe it will eventually be admitted, in the face of an unvarying experience, that these docked or curtailed members are comparatively useless. In the lower extremities certainly. The best apology or substitute for the old joints is offered by some of the resected fore-arms. These remarks are perfectly consistent with the fact, that where all the tissues are or have been diseased for a lengthened term, pieces of bone may be picked away, sequestra removed, or a sinus may be dilated to any reasonable extent often with advantage. The principle for which I contend is this: that anticipation and untimely meddling on the part of the surgeon are never justifiable, and always hurtful in the end. The disease in the articulation is a natural though a morbid process. When nature is ready by ulceration or necrosis or a sloughing of soft parts, to cast off a deadly incumbrance, then remove it with as little cost to the system at large as may be; but it can never be necessary, with this simple end in view, to perform an operation, compared with which amputation is a trifling infliction, in a vast proportion of otherwise remediable cases.”

We have here quoted from Mr. Travers at full length. This is too important a subject to be at the present day treated lightly; the question is one fraught with too much either of weal or woe to suffering humanity for us to allow so sweeping a condemnation to pass unnoticed in our pages—pages in which this class of operation has been fully discussed, and in which singular ability has been displayed in advocating under suitable circumstances its expediency—nay, its vast utility, and the great boon it confers on sufferers, who would otherwise be subjected to disfigurement and mutilation. The whole question is one that Mr. Travers himself has curtailed within extraordinary narrow limits—“Few of the successful cases sur-

vive long, to say nothing of those who sink within ten days or a fortnight after the operation of excision." Again—"Whatever may be said to the contrary by interested witnesses, I believe it will eventually be admitted, in the face of an unvarying experience, that these dooked or curtailed members are comparatively useless; in the lower extremities certainly." Now, without pausing to dwell on the extremely ungracious, nay, unprofessional sneer conveyed in the words, "*interested witnesses*,"—an expression as unworthy as it is disparaging, and one that we should not have expected from a gentleman of Mr. Travers' position, even had he not expressed himself in his preface as anxious not "to hurt the reputation of others"; though how he could more effectually stab the reputation of the distinguished advocates of this plan of treatment, from the earliest, Messrs. Park and Filkin, down to the latest, our own talented and distinguished fellow-countryman, Mr. Butcher, we know not; but the long roll of distinguished surgeons who have more or less identified their names with this mode of operative procedure—including such men as Park, Filkin, the Moreaus, Mülder, Fricke, Jaeger, Roux, Crampton, Syme, Fergusson, Jones, Mackenzie, Erichsen, Canton, Butcher, G. Porter, Tufnell, &c., &c.—requires not their names to be vindicated from so opprobrious a charge by our feeble pen. We shall therefore dismiss this most unpleasant portion of the subject from our consideration, and proceed to consider Mr. Travers' other allegations, which may fairly be epitomized as follows:—

- 1st. That of the successful cases, few survive long.
- 2nd. That the non-successful cases die within ten days or a fortnight after the operation.
- 3rd. That experience proves that, in such cases as survive the operation, the limb is comparatively useless; in the lower extremities certainly.

Now, these three are each in themselves serious charges, not to be lightly hazarded; so serious that, if established on sufficient data, the question of the impropriety of the operation would be at once set at rest for ever. But our readers may well ask what data are furnished us by Mr. Travers to decide this all-important question; and we fear much that they will scarcely credit us when we state that not one fact, case, statistical table, nay, one figure, is brought forward to justify him in this sweeping denunciation of a line of treatment supported by such distinguished operative surgeons as those whose names we have already quoted for our readers. His simple statement, his unsupported opinion, is to weigh more with the rising ge-

neration of surgeons than the recorded cases, facts, and statistics on the subject, with which our medical literature is now so rich. With reference to the first of his three propositions, we should like to be informed why or wherefore a patient who was convalescent after this operation should be more likely to sink into a premature grave, than after any other operation of equal magnitude. Where has he found any records to justify a statement so contrary to what might *a priori* be expected to be the true condition of facts? Were we not in a condition personally, on our own acquaintance with the true statistics bearing on this point, to give this axiom of our author's a most unqualified contradiction, we might rest content with this view of the question; but we do not think that it would be just, in discussing the merits of so important a measure as this, to base our arguments on any *petitio principii*. We will go further, and state that, from opportunities afforded us of witnessing and examining patients who have undergone this operation, we are in a position to assert, that so far as their future prospects of life are concerned, with reference to the resection of their diseased joints, they are as likely to live as any other members of the community; and that, from their present state of health, they are justified in entertaining sanguine expectations of a hale old age, relieved of the disease with which their life had been threatened. This statement may be objected to, as being but the expression of opinion of an anonymous writer. Well, be it so; but are there not diffused throughout the pages of our medical journals remarkable and repeated instances, verified by the signatures of their reporters, of the truth of this position? Is the united testimony of such men as Ferguson, Butcher, Erichsen, Brotherton, Humphry, Jones, Smith, &c., &c., all to go for nought, in deciding such important questions? Why should an operation, if anything involving less serious injury to the human organism than amputation in even the lower-third of the thigh—where, if performed with but ordinary care, the principal vessels are not wounded, and consequently less loss of blood is entailed, and the chances of secondary hemorrhage are to a very extraordinary extent diminished—where the line of incision is removed to a still greater distance from the trunk, where large veins are spared injury,—in fact, where the wound itself is of less magnitude than that of any ordinary amputation in the lower-third of the thigh—why, we ask again, should this operation, *a priori*, and in the face of documentary evidence of the very strongest and most convincing character to the contrary, be accused of being

ultimately more fatal than amputations under similar circumstances?

As to the second proposition, involving the death of the non-successful cases within ten days or a fortnight, we should like to ask Mr. Travers at what period, or how soon after amputation, do the fatal cases occur? But to meet him on this point would, in our minds, be but a fencing with a question of serious import to the community at large, and the scientific position that conservative surgery now occupies in these islands. Were we but discussing this question as between ourselves and Mr. Travers, we would at once end the discussion, by referring him for perusal and edification to the masterly essays on this subject that have appeared in the pages of this Journal from the pen of one far more capable of doing justice to this or any surgical topic—the uncompromising advocate and able exponent of this form of operation, Mr. Butcher. But as Mr. Travers, in his preface, states that his object is to secure for himself “the credit of having striven in his generation to arrest the progress of error and enlarge the bounds of truth,” we, with a similar laudable intention, shall be permitted to extract from Mr. Butcher's masterly essays the following summary of cases, amply and ably recorded by him for a similar object:—

In Mr. Butcher's first memoir on excision of the knee-joint, we have, in a tabular arrangement, an account of thirty-one cases, of which five were fatal, and twenty-six recovered; and in his second memoir on the same subject, fifty-one cases are recorded, of which number ten were fatal, and forty-one recovered; making a sum total of fifteen fatal cases, and sixty-seven recoveries, out of eighty-two cases subjected to the operation. From the number of recoveries, however, we have to subtract six cases, in which amputation was had recourse to as a secondary measure, which will leave on record a sum of sixty-one recoveries, as against twenty-one failures; or, in round numbers, we may state that three-fourths of the number of cases operated on, or seventy-five per cent., were successful, and from most trustworthy sources proved to be so. And yet we find Mr. Travers, in the year of grace 1860, coming out with the sweeping general assertion already quoted in these pages, and not vouchsafing us one figure, one fact, nothing beyond his own mere *ipse dixit*, to support his statement and deliberately recorded opinion. We care not here to pause to inquire, might not a more careful analysis of these unsuccessful cases, with a view of attributing the fatal issue to its first cause, be attended with the result of even yet more reducing

the number of those fairly to be attributed to the operation itself. We seek not (though in justice, were we discussing the merits of the operation, we would be bound to do so) to inquire how many of them died from pyæmia, extensive bed-sores, organic disease, hemorrhage, phlebitis, shock, delirium, &c.—all of which complications tend to swell the fatal numbers of even the best accepted and most recognised surgical operations. We, for argument sake, will grant that each and all of them are with justice to be ascribed as a natural consequence of the form of operation selected; and still we assert that it will bear favourable comparison with amputation of the thigh. In this operation the mortality is scarcely twenty-five per cent: has any one authority in surgery ever yet assumed as low a rate for that succeeding amputation?

We shall now proceed to examine how correctly Mr. Travers has stated that "experience proves, in such cases as survive the operation, the limb is comparatively useless; in the lower extremity certainly;" and, to enable us to do so, we shall again have recourse to Mr. Butcher's most valuable and masterly analysis of the result of this operation. In thirty-one of the cases in these tables, we find these words appended, "**WITH PERFECT USE OF THE LIMB**"; and this exclusive of many others exhibiting a most satisfactory condition of the limb, leaving but little doubt on any candid or unprejudiced inquirer's mind, that they also were, or shortly would be, additional triumphant examples of the extraordinary boon conferred by modern conservative surgery on these forms of disease. Thirty-one cases, in all of which under the old *regime*, the unhappy being would be a maimed object for the rest of his life, even if that life had been spared in the fearful game of equal main and chance which had been played for him, but in which now the patient has a *perfectly useful limb*, has the symmetry of his body preserved, and is not dependent on mechanical skill for the means of progression, or the preservation of his appearance\*.

Some people, however, Didymus-like, require to have their senses convinced, and will not accept anything on mere assertion. Surely such proof was easily to be procured for Mr. Travers' sceptical mind; the brilliantly successful cases of Professors Fergusson, Erichsen, Bowman, &c., &c., were open for investigation, and *should have been examined, and carefully examined*, ere he dare to venture to question the merits of an operation now firmly established on such high authority and such unquestionable statistics. Such of us as have had in this city the opportunity of seeing and examining Mr. Butcher's

\* See also Dr. Canton's first case, at page 76 of our present Number.



most successful cases, Mr. George Porter's most interesting case of excision of the elbow-joint for injury, and Mr. Tufnell's case also of a similar operation for an analogous accident, will not allow our judgments to be warped by any rash, ill-judged, and unfounded criticism on the merits of this form of operation; but to such as have not been so fortunate, it becomes our duty to warn them to reserve their judgment, to be most cautious in condemning an operation of which personally they have had no experience, and of which consequently they are only entitled to form an opinion from the writings and statistics of those who have studied the question, and are competent and entitled to speak on it with authority—to place for one moment in serious juxtaposition the advantages to be derived from an operation that preserves in their perfect integrity the motions of all the articulations below the one that has been excised, and that in which all these, together with the diseased joint, are sacrificed, is simple inanity; whilst, to claim *superiority* for that in which artificial means must supply those of nature, is but to assert that the work of man is greater than that of God. The question can, nay, ought, only to be discussed on this one great ground. On which side do statistics prove the greater mortality to exist? And we think that we have justified ourselves to our readers in stating that this point has been decided on most abundant and satisfactory evidence to be in favour of excoision.

That a gentleman who has taken exception to the propriety of the operation of resection of joints—an operation of English growth, warmly advocated, extended, and improved by English surgeons, aided, as we feel they will gratefully acknowledge, by Irish talent and industry,—should attempt to cry down one of merely Irish growth, is not to be wondered at. Therefore our readers must not be surprised to hear that the treatment of aneurism by compression comes also under Mr. Travers' lash. We are so anxious to avoid even the semblance of injustice in this gentleman's case that we shall submit his own words for examination:—

“*Magna est veritas!* The universal adoption of the ligature as a cure for aneurismal swellings, and the comparative certainty of its success in properly selected cases, is the best excuse which can be offered in explanation of our views as pathologists; and it justifies the conviction now commonly avowed by all operators in this country, that neither the presse-artère, nor any amount of external compression, whether mediate or immediate, is worth the pains and risk attendant upon such indirect and comparatively imperfect modes of procedure. Exceptions, however splendid, never justify a rule.”

It is true that this lecture was delivered so long back as 1847 to the surgical class of St. Thomas' Hospital; but can this plea in abatement be urged with any justice for the statement being put in print in the year 1860, without one word of explanation or qualification? Before 1847, the entire surgical profession in these countries had ample opportunities afforded them of studying the relative merits of ligature *v.* compression. Bellingham had written and taught, as also Tufnell, on this most important subject; numbers of cases had been given illustrating the vast advantages of compression as contrasted with deligation; and at the present hour to question in suitable cases the respective values of the two modes of procedure, is worthy of those alone who write as Mr. Travers has done on resection. The only excuse for his so doing is, as we have already intimated, that the lecture was delivered at a period when English surgeons were not unanimous on this great Irish improvement; for even in these days of steam and telegraphs, it takes a long time for a fact to reach St. Thomas' Hospital from the City of Dublin or St. Vincent's Hospitals; but to write now as he has expressed himself in this passage puzzles our inventive faculties for explanation. It may have been done with the view of giving us an historical example of the difficulty under which truth labours for its diffusion: just as Stephenson's "Coo" is quoted in works recording the obstacles thrown in the way of the introduction of our present system of railroads; but what would any child of the present day think of the engineer who would gravely advance as an argument against a locomotive the danger that would accrue to it did a cow crossing the line come in contact with it? We rather think that he would be inclined to agree with Stephenson that it would be "a very bad thing—for the coo." We repeat that the objection is of interest in an historical point of view; and so likewise would be Mr. Travers' reference at the present moment to an opinion expressed thirteen years ago on a point of surgical treatment at the time with many able English surgeons still *sub judice*; but gravely to repeat it now, argues a want of progression with the science of surgery that would be laughable, were it not at the same time so very much to be deplored in the person of one aspiring to teach our future race of surgeons.

It is not our intention to attempt the vindication of the treatment of aneurism by compression. At the present day, the surgeon who questions the value of this plan of treatment cannot be allowed to do so on his mere assertion: facts and arguments will be looked for by surgeons at his hands; and if

he be not prepared with such, his opinion will be just received at as much as it is worth. But we cannot resist reminding our readers of a passage that occurs in Mr. George Porter's most valuable "Contributions to Operative Surgery," that appeared in our number for last November. This passage, extracted from the latest production on the subject, and emanating from the pen of a surgeon that all who have witnessed his practice in the surgical wards of the Meath Hospital will acknowledge not to be timid or unskilful in the use of the knife—that "last appeal" of every sound surgeon,—must be accepted as the exposition of the surgical treatment of this most important class of diseases at the present period:—

"In the meantime, a case of popliteal aneurism was cured in the Richmond Hospital by mediate compression; another occurred in Jervis-street Hospital; and soon this method of treatment came to be ratified and confirmed by a course of success so unvarying and continued, that most surgeons began to regard the operation of securing the femoral artery as little more than a matter of bygone history, never more to be resorted to in the treatment of popliteal aneurism.

"But in medicine there is no proposition universally true, —no mode of treatment applicable to every possible case. Doubtless, the operation of deligating the femoral artery at the usual place has totally fallen into abeyance, and probably has not been twice performed in Dublin during the last fifteen years; whilst the treatment by compression has been generally so successful, and has so entirely won the confidence of the profession, *that very good and sufficient reasons would be required of any practitioner proposing to deviate from it.*"

The paper on anthrax is written to advocate the value of the exclusive use of potassa fusa in the treatment of this form of disease. As an auxiliary, we are willing to accept it; but to depend on it, to the complete exclusion of other modes of treatment, is what no sound surgeon will consent to do. The entire subject of anthrax and its treatment, however, has been some short time back (in our number for November, 1856, vol. xxii.) so ably handled, in a most masterly paper on the subject by one whose untimely loss Irish surgical literature is still deploring—Mr. T. H. Ledwich—that we must content ourselves by referring our readers to a reperusal of that memoir, in which they will find this and other plans of treatment contrasted and discussed in a most exhaustive manner, and for which Mr. Travers' present paper would indeed prove but a very poor substitute.

In heathen mythology, the god Mercury has been accused

of many a trick, and had many a sin laid at his door, but we doubt if his greatest detractor in former times ever brought a stronger charge against him than this of Mr. Travers:—

“I knew a very fat, jovial woman, fresh-coloured, past 40 years old; she had borne several children, and never had an hour's illness in her life. One day she showed her ordinary attendant a small hard tumour in her left breast, which was plump and well-formed. The induration did not extend beyond the lump, which was as hard as a stone, and moved freely in the substance of the mamma. Amongst other remedies, this patient was ordered to try a five grain plummers' pill every night and sarsa. She had not taken more than a dozen pills when she was attacked with acute pleuropneumony, of which she died rapidly. I saw the case myself, and I observed at the time that I thought the mercury had killed her.”

Many ills have we known to arise from the injudicious use of mercury; many woes have been attributed to its abuse; but gravely to ascribe a fatal attack of pleuropneumony to the dose mentioned here, is certainly rather too severe a tax on our credulity. The *post hoc propter hoc* theory never has been better illustrated than in the present instance.

In concluding our notice of Mr. Travers' work, we must express our regret in having been forced, in the strict discharge of a public duty, to speak of it in such harsh terms. We opened it with a most favourable impression, impressed with the author's hereditary claims on the respect and kindly feelings of every true lover of surgical science; but we are much deceived in the estimate we have formed of the character of the late Mr. Travers, if he (were he compelled to criticise it) would not himself, in vindication of surgery as a science, act with as stern a sense of duty as in former times, in defence of social order, actuated the Warden of Galway.

*The Surgical Diseases of Children.* By J. COOPER FORSTER, F. R. C. S., &c. London.—Parker: 1860. 8vo. pp. 348.

WHEN an author brings out a work on a practical subject, the first inquiry we naturally make is, what his opportunities have been for acquiring experience. From the title-page of the book before us, we perceive that Mr Forster is Assistant-surgeon to Guy's Hospital, and Surgeon to the Royal Infirmary for Children, so that we may reasonably expect him to be well qualified for the task he has undertaken. The surgical wards of

Guy's Hospital receive, we believe, more than three hundred children annually, and by children patients under puberty are meant. In the preface we are told that "the entire matter of this work is original,"—that it contains the result of the author's own experience; and where his own observation does not afford any data, he is silent. It is not his aim, then, that this should be a complete systematic treatise upon the surgical diseases of children; and accordingly we find some subjects altogether omitted, as diseases of the eye—and others, as hernia, and injuries of the head, &c., but slightly dwelt on, as not presenting in the case of children any differences either in character or treatment from the same diseases in the adult. Above, when it is stated that the matter of the writer is "original," it is not to be supposed from this that the author's views and modes of treatment are new or original—in this sense, there is little of originality in the book—but merely that he tells us the opinion he himself entertains, the principles that have guided him, and the plans of treatment he has himself followed, irrespective altogether of their originators, and without any reference to "authorities." To our minds, a book of this kind is much more attractive than any compilation; and, provided its author be a man of information and experience, is much more profitable to the practitioner.

In all operations, even of the slightest kind, Mr Forster administers chloroform, considering it to be so safe in the case of young children. He believes that as children advance in years, the danger of chloroform increases; but he admits to having heard of a "very few" fatal results, when employed in early life. He very properly enjoins that it should be given on an empty stomach, to avoid sickness; and the sensitiveness of the conjunctiva is the test on which he relies, for indicating when the patient is fully under its influence. Freezing mixtures (for producing local anæsthesia) he considers inapplicable for very young children; but that in children verging on puberty, this agent may be most advantageously employed, in the opening of abscesses, and other cases in which it is desirable to abolish the cutaneous sensibility. As this kind of local anæsthesia is not as much employed, we think, as it might be, we here borrow Mr Forster's directions.

"The best mode of applying cold is to mix thoroughly together equal parts of ice, broken into very small pieces, and of salt, and place them in a piece of very thin caoutchouc sheeting. This should be gently drawn to and fro over the part for two or three minutes, until the skin begins to assume a whitish appearance. In this state, if the skin be incised, it cuts like a raw turnip and without the

least sensation to the patient. Some little uneasiness attend the first application of the freezing mixture, and also its withdrawal. The latter may be somewhat relieved by making the withdrawal gradual. To avoid sloughing, care should be taken that the application be not continued too long."

We cannot possibly notice all the different subjects treated of by Mr. Forster in the pages of this work; indeed, they comprehend nearly all the diseases and accidents of early life, calling for surgical treatment. Some of them, however, we shall notice, in order to give the reader the benefit of his remarks, or to make them the subject of criticism.

*Epistaxis*, though a very common, and sometimes a serious complaint, yet, in the experience of Mr. Foster, has never proved fatal, or even dangerous; as on no occasion has he ever had to employ plugging of the nostril.

*Cancrum oris* he regards as a disease distinct from gangrenous stomatitis, with which it is often confounded, and under which appellation often described. But when we find so high an authority as Dr. West so designating it, it may well be supposed that the difference, if any, between the two complaints, is very slight. The question of whether, or how far the administration of mercury may have to do with the production of this terrible malady, has, we believe, been decisively settled in the negative. That a gangrenous affection of the gums and cheek has supervened upon the use of mercury with children, is an undoubted fact, but one of excessively rare occurrence, and attributable to accidental coincidence, or to a peculiar idiosyncrasy of the patient, just as the same destructive process may attack adults using this mineral. In the last (28th) volume of the former series of this Journal, Dr. James Duncan has published a most able essay on the disease, and has fully entered into this question; and he has produced a large amount of evidence tending to prove, in the most conclusive manner, that *cancrum oris* may, and most frequently does, arise altogether independently of the use of mercury. In opposition to all this, Mr. Forster "is disposed to believe that the indiscriminate use of mercurial powders, a portion of which is very frequently retained in the mouth of young children, is, in the larger proportion of cases, the exciting cause" of the disease; whilst previous depressing maladies, badly-ventilated dwellings, poverty, dirt, starvation, &c., &c., he regards as predisposing causes.

*Hare-lip*.—With respect to the time for remedying this defect by surgical treatment, the author's experience is decidedly in favour of "operating at the earliest possible period; remembering, however, that loss of blood in very early infancy

may be attended with the worst consequences." He would have no hesitation in operating immediately after birth, and has successfully done so within seven hours. His principal reason for preferring to incur this greater risk from hemorrhage, in the very early operation, is, that the child may be enabled to suck. The danger from the operation he believes to be less than that from the imperfect nutrition which results from the deformity. In the main, he is right, we think; and the large majority of these children are physically in a much better condition for undergoing the operation soon after birth, than for months afterwards; nay more, in some of these cases, where the deformity is very great, at no subsequent time is the child in such good condition for being operated upon; and, in a few instances, if this period be allowed to pass over, the opportunity for operating is irretrievably lost; for the child so rapidly falls away in health, that no prudent surgeon would think of subjecting it to any serious operation. Mr. Butcher—no mean authority upon this subject—gives a preference to the early operation, but "prefers waiting a few days after birth, in order, as it were, to allow the functions of the body to be healthily in action\*." This reasoning is, perhaps, rather fallacious; for, in nineteen cases out of twenty, children, on their entrance into the world, are in far better bodily health than they are for a long time afterwards; and, in corroboration of this, we would mention the fact, that nearly all infants lose weight the first week after birth.

The various accidents to the larynx and trachea, which are apt to require tracheotomy, are clearly described, as well as the operation itself. Upon the much-debated question of the utility of this operation in croup, Mr. Forster offers no opinion, very cautiously declining to enter into the controversy. This reticence on his part we much regret, and rather deprecate. As the author professes to give us in this work merely his own views and experience, it was reasonable to expect an expression of his opinion on this great practical question. That he has had considerable experience of the measure is plain, from the little information he vouchsafes relating to it. "I have performed the operation (he observes) in various periods of the disease, and with varied success." As to the particular period at which it should be performed, he thinks that the advanced stage of the disease is the best time; nay, he even "doubts much whether when a patient is moribund be not the most advisable time for the proceeding;" and, in illustration, reports

\* See his paper on Hare-Lip, in the twenty-first volume of this Journal.

the case of a boy four years old, on whom he operated with complete and permanent success, and yet the symptoms presented by the child before the operation were such as indicated immediate dissolution—perfect insensibility, face and lips quite livid, faint efforts at respiration, but the air scarcely entering the chest, the pulse imperceptible, and the skin cold. This case is entitled “Laryngitis;” and, perhaps, the question may be raised, whether it really was an example of genuine croup; certainly it does not appear that any false membrane was formed in the trachea or its subdivisions.

Diseases of the urinary and generative organs, in both sexes, are discussed at considerable length. These chapters, four in number, display much practical knowledge, and will be consulted with profit and advantage by all surgeons who are liable to be called in to attend upon the young; and this takes in, we believe, all *civil* surgeons.

*Infantile Leucorrhœa.*—In connexion with this disease, the author alludes to the frequent supposition of its being of a specific kind, and communicated by the foul contact of some person affected with venereal disease. He admits the impossibility of distinguishing gonorrhœa, by any pathognomonic symptoms from some cases of infantile leucorrhœa; but, at the same time, he is of opinion that, as a general rule, not the slightest importance is to be attached to the allegations of the friends of the child as to the origin of the complaint, and that it is certain circumstantial evidence only, of the most unequivocal kind, can lay any basis for the imputation. The statements made by the children themselves are entitled, he thinks, to very little reliance. In all this, he only corroborates what has been written on the subject by others, and especially by Dr. Wilde, to whose papers he alludes in terms of strong commendation.

*Calculus in the urethra.*—Apart from external injury, this is almost the only cause of retention of urine in the child. In these cases, the inability to pass water is the first symptom; there is no history such as we find in stone in the bladder. Generally speaking, by the time the surgeon is called in, the bladder is considerably distended, and may be felt above the pubes, and through the rectum. In such a case as this, the treatment is, according to our author, “first of all, to place the patient under the influence of chloroform.” On this, and another point, namely, the mode of removing the calculus from the urethra,—we must let Mr. Forster speak for himself:—

“Only actual experience can enable us fully to appreciate the value of chloroform, and the instantaneous relief it gives, not only



in some instances of retention of urine, but in numerous other affections of children. It is not, however, always the case that the calculus is expelled on the administration of the drug; and if the retention continues, a catheter must be passed. In doing this, the calculus may be distinctly felt, and accidentally pushed back into the bladder, where its presence may, or may not, be afterwards detected by the sound. . . .

"If not contained within the prepuce, the calculus may be found at any other part of the urethra, but most frequently about the bulb. Various plans may be adopted for its removal, according to the part of the canal at which it is lodged. If just within the meatus, the orifice may be slit open, and the calculus abstracted. If situated in any other part of the urethra, a long narrow pair of forceps has been recommended to be passed down, and the stone seized. This, however, is an unsatisfactory proceeding in children; and I have never adopted it, believing that it is fraught with less danger and injury to the canal to pass a grooved staff down to the stone, and cut into the urethra in the mesian line from the perineum, or from the under part of the penis, either in front of, or behind the scrotum. I must advert here to a maxim that is inculcated, but which I am satisfied is incorrect, viz., that the urethra should never be opened in front of the scrotum. I have opened it there myself, and seen the same thing done by others, and the wound has healed quite as rapidly as when the incision was made in the perineum."

One entire chapter is devoted to the subject of calculus in the bladder, and a description of the operation of lithotomy. It is full of sound practical instruction, and may be read with advantage by all. Indeed, these chapters on the diseases of the genito-urinary organs (especially in boys) are among the best in the book, and give the results, in very plain language, of large clinical experience.

*Paraphymosis.*—The author's plan for replacing the prepuce, in this disease, is "to place the boy on his back, to grasp the penis firmly with the right hand, and proceed to lift him bodily up." He has found this mode of reduction so successful, that he now resorts to no other. We think that the inhalation of chloroform would be very proper, before subjecting the child to so painful a proceeding.

*Nævus.*—Passing by the chapters on diseases of the testis, and on hernia, we come to that one on nævus, and feel bound to say that it contains a very comprehensive and explicit description of this disease, and of the different modes of curing it, with occasional remarks upon the comparative value of these various plans. Departing from the course which he elsewhere pursues in his work, viz., of strictly confining his observations

to the results of his own experience, he here gives a very good and clever resume of all the methods (about sixteen in number) at present in use for the cure of *nævus*. As we have heard a good deal lately about the injection of *nævi*, we shall extract some of his remarks thereon:—

“Until the recent introduction of the perchloride of iron into surgical practice, by Malgaigne, the operation of injection for the cure of *nævi* was comparatively unsatisfactory. This substance, however, if used in certain picked cases, is so efficient, and with proper precautions is so safe, that it has, in my opinion, given the procedure by injection a place among the best remedies for this affection. The cases, however, must be carefully selected, or disappointment will ensue. When the *nævus* is of the true subcutaneous type, the skin not being involved, and when it is situated in a conspicuous part of the face, neck, or arms, where a scar is to be avoided, injection of the perchloride of iron is the plan I always adopt. But these cases, it must be remembered, are not frequent. The mode in which I perform this operation is as follows:—First, introducing through the skin a narrow double-edged knife, about half an inch from the apparent margin of the *nævus*, I pass it into the substance of the growth, and move it in all directions to divide the tissue. If the *nævus* be large, I do this from several points. As the knife is withdrawn, there is a flow of blood, which the pressure of the finger will control. Then, the syringe being previously charged with the solution of perchloride of iron, I introduce the point of it into the aperture, or into each aperture in turn, if there are more than one, and throw into each opening about four or five minims. Coagulation of the blood instantly takes place; and the tumour becomes hard and prominent. Absorption gradually ensues, and the structure is destroyed. If the injection penetrate the whole *nævus*, as evidenced by the hardening of the whole, no further operative procedure will be necessary; if, on the contrary, any portion retain its softness, that part should be injected through a fresh opening, as soon as convenient. When the absorption is complete, no trace is left to show where the disease has been; but this process may occupy several months, or even a year.”

The succeeding chapters of the book are upon injuries and diseases of the bones, joints, ears, &c., diseases of the skin and nails, congenital deformities and malformations, tetanus, burns and scalds. Though the remarks on these are generally very brief, yet they are practical, and to the point.

From the extracts we have given, and the comments already offered, the reader can be at no loss to know our opinion of this work. Although this is, on the whole, most favourable, still we think that on some subjects the author might advantageously extend his remarks; whilst some of the subjects on which

he has treated (e. g. diseases of the skin, infantile syphilis, scalds, burns, &c.), might be omitted altogether, not presenting sufficiently distinctive features in children to merit notice in a work of this kind. We cannot close this notice of Mr. Forster's work, without mentioning that it is abundantly illustrated with well-executed wood-cuts and coloured lithographs.

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*The Principles and Practice of Surgery.* By WILLIAM PIRRIE, F. R. S. E., Professor of Surgery in the University of Aberdeen, &c., &c. Second Edition. London: Churchill, 1860. 8vo, pp. 880. Illustrated by numerous engravings on wood.

AMONGST the books which have been issued from the press during the past month, we have the second edition of Mr. Pirrie's treatise on surgery. At a glance, the volume is an attractive one; but when we come more closely, more particularly to study and analyze its contents, we are struck by the concise, practical way in which the different subjects are handled. Eight years have just elapsed since the appearance of the first edition; and certainly the author seems not to have spared time, labour, or money in enlarging, illustrating, and bringing out a work in every way commensurate to the wants of the student, and furnishing an admirable epitome of the surgery of the day. Many chapters have been entirely rewritten, and others greatly enlarged and improved. On contrasting the bulk of the volume with its predecessor, this may not appear; but on nearer examination it will be seen that the type is much smaller, and more closely set; while the most beautiful woodcuts adorn almost every page, each of them being a faithful exponent of the text. We could not afford room to enter into the merits of the different sections into which the volume is divided, as otherwise we would be inclined to do; but this is the less required—indeed, the necessity for so doing is negatived in regard to a book which has passed so rapidly through its first edition. Each chapter, according to the import of the subject, seems to have called forth the energies of the writer; and, as a sequence, there are some to which we would wish particularly to direct attention, as being replete with information, condensed in the most suitable way. For instance, the exposition of the subject "Aneurism" is clear, most instructive, and perfect throughout. And the same

truthfulness which stamps the book, accords without any petty, narrow-minded jealousy, the credit and the honour which redound to the Irish school for the establishment of its treatment by compression, as a practice which, in the author's language, "is a safe, simple, successful, and almost painless mode of curing aneurism." He justly associates with Bellingham's name the important truths revealed in its cure:—

"When Bellingham first called attention to this interesting subject, he stated as his opinion, that it would be unnecessary to employ such a degree of pressure as would cause inflammation and obliteration of the artery at the seat of pressure; but that it would be sufficient merely to weaken the circulation through the artery and the sac, thereby favouring consolidation by the deposition of lamellated coagulum. In cases treated successfully by this mode, opportunities have occurred of making *post-mortem* examinations, in consequence of the fatal results of other diseases; and it must have been gratifying to Bellingham to find, that in most of these instances the main artery was pervious everywhere except at the aneurism."

And again:—

"The late Dr. Bellingham, one of the surgeons of St. Vincent's Hospital, Dublin, had the merit of having suggested this new mode of using pressure, and of having proved its success, as well as of bringing the subject before the profession. He treated a considerable number of cases with perfect success, and his method has been practised with equally gratifying results by other surgeons in Dublin and elsewhere. Bellingham stated, in regard to the favourable impression entertained of this mode of treatment, 'So highly satisfactory has been the result of compression in Dublin, that no surgeon of that city would in the present day perform the operation of applying a ligature to the femoral artery for popliteal aneurism.'"

It would be unjust to the clear discernment of the author, if we did not quote his concluding sentence in reference to this subject. It may be taken as a test for his truthfulness in other statements:—

"The surgeons of Dublin can have no possible motive for preferring compression to operation, except the conviction that it is the preferable mode of treatment. In operative surgery, 'whatever men dare, they can do,' as well as hands can do; but in suitable cases they prefer compression to operation, because they believe it to be the safer mode of treatment."

The subject of phlebitis, the very name of which sounds alarm in the ear of every surgeon, whether it steals on insidiously as an idiopathic affection, or suddenly strikes with

death the sufferer after operation, is likewise dealt with in a masterly way. All that is known about it—its cause, its course, its management, its pathology, have each been considered with research, acumen, and judgment.

There are other chapters, also, to which we would wish particularly to refer. That on hernia is especially interesting and practical in all its details; and this section is most elaborate, occupying nearly fifty pages.

We have a concise but brief account of resection, this conservative effort of our art, as applied to several articulations. The author gives the result of two cases of excision of the wrist-joint, occurring in his own practice, attended with the best results:—

“I have twice performed the operation of excision of the wrist-joint; once in the Aberdeen Hospital, and once in private practice. In the latter case I operated according to Mr. Butcher’s method, and the result was a serviceable hand, with three of the fingers decidedly bent. In the former case, I made a long and straight incision behind, deflected the tendons to each side, and removed the whole of the carpal bones, along with the extremity of the radius; and the gratifying result was, that the patient had a most serviceable hand, and enjoyed the full use of all her fingers.”

From the careful way in which this second edition has been produced, we have no doubt that it will shortly be exhausted; and we look forward to a third edition as the immediate consequence. No book in its early editions can come up to perfection; so far as it is possible, Pirrie’s Surgery will take its stand now. Within the last few years, surgery has made such rapid strides, and is still so progressive, that the author will constantly be apprized of the necessity for additions; and we shall look forward with confidence to the third edition of this valuable work, and put our trust for the consummation of our wishes in the zeal and ability displayed in this second edition. The clearness and precision of Professor Pirrie’s descriptions have thrown a charm over the entire work; and the honest high-mindedness with which he accords to every man what his researches and labour may have brought to practical utility and worthy of notice, calls for our highest commendation; it being too much the custom of authors of systems of surgery, in the present day, to seize upon the opinions and writings of others, and put them forth clothed in their own language, without reference to a name, or acknowledging the sources from which in reality they were procured.

In conclusion, we can assure the learned author that we

have read with pleasure every page of his book, and heartily recommend it to the student as a volume best fitted to qualify him for those competitive examinations which are the order of the day. But does its usefulness rest here? Certainly not. The practical precepts laid down upon all critical questions must enhance the value of the book, and render it indispensable to the practising surgeon. It should be conspicuous in his library shelf for reference, and we pledge ourselves it will not fail him in his necessity.

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*On the Reparative Process in Human Tendons, &c.* By WILLIAM ADAMS, F.R.C.S. London: Churchill. 1860. 8vo, pp. 175.

IN the twenty-fifth volume of our present series we had occasion to notice, with approval, Mr. Adams' sketch of the "Principles and Practice of Subcutaneous Surgery,"—an essay which, in spite of some trifling faults of style, gave evidence of careful observation and sound reasoning. In the present work, which follows appropriately on the former, we rejoice to observe the same close powers of observation, with an absence of anything disagreeable in the style. In it will be found a series of observations on the union of tendinous structures, carefully noted and compared; there is also a profusion of excellent illustrations, both of the physical characteristics and of the minute anatomy of the new material, at periods varying from four days to three years after division. In addition, there is a series of experiments on rabbits, with a resume of the English and foreign literature of the subject. The work should be on the shelves of every operative surgeon who has claims to a scientific acquaintance with his profession. No doubt, it can add little to our knowledge of the art of surgery; but to those who love to go deep into its principles, such contributions are most acceptable. In the hope of inducing such of our readers to become personally acquainted with the work, we copy a few of the observations and conclusions of Mr. Adams. Before doing so, we have to remark, that the previous literature of the subject, commencing with John Hunter, deals rather with experiments on animals, than with observations derived from the human subject. Owing to the wide difference of the results obtained by successive writers, Mr. Adams instituted, in the year 1855, a series of thirty-two experiments on rabbits—of these he remarks—

"I will here only observe that my investigations generally confirmed the account of the process previously given by Mr. Paget, espe-

cially with regard to the influence of the blood and the inflammatory process in interfering with, rather than assisting, the reparative effort; in this respect, therefore, they were at variance with the opinions of all the other authorities above mentioned [Hunter, Mayo, Von Ammon, Pirogoff, Gerstaecker, Thierfelder, and Bauer]. My investigations also confirmed Mr. Paget's observations with regard to the development of the new connective tissue from a proper reparative material, effused for this purpose, and described by Mr. Paget as 'nucleated blastema,' a material in which the cell-forms do not advance beyond the condition of nuclei, and thus differ from inflammatory lymph, in which fibro-cellular tissue is developed from nucleated cells. The most material point in which my observations differed from those of Mr. Paget, was in reference to the influence of the sheath of the tendon, which I found not divided, and generally very little injured, in the subcutaneous operations. It therefore appears to me, that in the reparative process the sheath was of primary importance in maintaining a direct connexion between the divided ends of the tendon. In my experiments, also, it appeared that the new reparative material was infiltrated between the fibrous elements of the sheath, which therefore at once formed the matrix for the newly-formed tendon, and also determined its direction and definite form, as described by Thierfelder.

"Mr. Paget, on the other hand, completely ignores the influence of the sheath in the reparative process; and, after describing the general infiltration and succulency of all the tissues surrounding the tendon, and the subsequent changes which occur in any inflammatory lymph and blood which may be effused, and the manner in which the development of the nucleated blastema laid down between the separated extremities of the divided tendon appears to proceed, observes that 'a single well-designed (? defined) and cord-like bond of union is thus gradually formed, where at first there had been a uniform and seemingly purposeless infiltration of the whole space left by retraction of the tendon.'

"Mr. Paget also states, that in experiments in which the tendon was divided by an open wound, when the wound through the integuments healed quickly, the case proceeded like one in which the subcutaneous division had been made; and therefore he adduces this in proof 'that it is unimportant for the healing of divided Achillis tendons whether the cellular sheath or covering of the tendon be divided or not.' The facts and observations which have led me to a different conclusion on this point, will be specially adverted to in the general description of the reparative process given below."

For these, however, we cannot make room, and shall pass on to the more interesting and unique collection of observations on human tendons.

The tendo Achillis of a child was divided, Oct. 28th, and on the 1st of November the child died of bronchitis. In this case—

"The divided extremities of the tendon were three-quarters of an inch apart; but a direct connexion between them still existed by means of the cellular sheath of the tendon, which appeared to have been but little injured by the operation, and passed between the divided extremities in a tubular form. The sheath was highly vascular between the divided extremities of the tendon; and a close net-work of congested vessels ramified through its structure, which was a little increased in thickness, and slightly succulent in appearance, from infiltration into its cellular meshes. The vascularity extended above and below only for a short distance beyond the divided extremities of the tendon; and the surrounding fat and cellular tissue presented a blood-stained, vascular, and infiltrated appearance. . . . The divided extremities of the tendon were square, abrupt, and unaltered, either in appearance or texture. It might, perhaps, be said, that the edges were slightly rounded; but the general appearance was that of a section just made."

In another case, the little patient sank from diarrhœa, eleven days after operation:—

"The divided extremities of the tendo Achillis were seven-eighths of an inch apart; but were directly connected by a solid bond of union, equal in diameter to that of the tendon, but of soft consistence, and externally of blood-red colour, partly from blood-staining, and partly from vascular injection. The surrounding fat and cellular tissue also presented a vascular and blood-stained appearance, and were infiltrated with inflammatory effusion.

"By a little dissection, I was enabled easily to define the connecting bond of union, the contour and definition of which evidently depended upon the cellular sheath of the tendon, the fibrous bands of which, now taking a longitudinal direction, were readily distinguishable. The substance of this connecting bond presented, on section, a somewhat gelatinous appearance, of a reddish colour, partly from blood-staining, and partly from vascularity. There was nothing like separable blood-clot anywhere, and very little blood appeared to have been effused at the operation in this case. Both the cut extremities of the tendon presented a square, abrupt outline, the upper being only somewhat rounded at its margins. They retained their dead-white colour, and natural texture; perhaps it might be said, that they were slightly softer than natural, and thus contrasted strongly with the blood-stained gelatinous texture of the new connecting material; with this material, however, they were pretty firmly connected, though, to the naked eye, there was no appearance of the dove-tailing process of junction, so obvious in the specimens at a later date."

The microscopic appearances in this new material were those of nucleated blastema. Omitting many other observations of intermediate periods, we pass on to the description of two ten-



dons, which had been divided three years before the death of the patient afforded an opportunity of inspection; here—

“The divided extremities of one of these tendons were found to be an inch and a quarter, and the other one inch and an eighth apart, but firmly connected by a firm bond of union, or new tendon, equal in bulk and thickness, to the extremities of the tendons it served to unite, and presenting a greyish translucent appearance, by which it was readily distinguishable from the old tendon. The new material had, in a more marked degree than I have seen in any other human specimen, a longitudinally striated appearance. The cut extremities of the old tendon were very distinct, from the contrast of colour above described; and, the line of junction with the old tendon indicated by a process of fine dove-tailing. The new material extended, at its circumference, slightly beyond, or, it may be said, slightly ensheathed the extremities of the old.”

Microscopically, there was no difference between the new and the old tendon.

In some dissections, Mr. Adams found a remarkable divergence from the above chain of phenomena, in the case of tendons enclosed in dense fibrous sheaths of a tubular form. Where tendons are so circumstanced, as is the case with the *tibialis posticus*, behind the ankle-joint, there appears to be a danger that the divided extremities will contract adhesion to the inner surface of the sheath, and that no direct bond of union between them, or new tendon, will ever be formed. We have lately seen a case of rupture of this tendon in this situation, in which, at the end of a month, no proper union, or attempt at it, had taken place; and, as far as one case goes, this corroborates Mr. Adams' views.

These observations serve to show that the function of the sheath has much analogy to that of the periosteum in the case of fracture, namely, that it mainly, but not entirely, furnishes the material for repair, and maintains the vascular connexion between the parts; and we have a practical rule deducible, namely, that extension should not be applied too early in the case of tendons which have but slight vascular connexion with their sheaths, as, in such cases, non-union is apt to occur, if there be too great separation of the extremities.

There is a controversy we hope an amicable one, between Mr. Adams and his colleague, as to whether the new material, which both parties agree to be poured out for the repair of divided tendons, remains as permanent new tendon, or gradually contracts until it approximates their ends, and forms a linear cicatrix. The latter theory is not well supported by ob-

servations, but is more in accordance with the general theory of the reparative process. Whereas, Mr. Adams advances numerous and careful dissections in support of his views, sufficient, as the question at present stands, to counterbalance their novelty, if not absolutely to establish their correctness.

*On Gout ; its History, its Causes, and its Cure.* By WILLIAM GAIRDNER, M. D. Fourth edition. London: Churchill, 1860. 8vo, pp. 630.

WE have, after a conscientious sifting of the most reliable data, arrived at the very impartial conclusion, that Gout *en grand*, Gout rheumatic, Gout aponeurotic, Gout muscular, and all the other and several Gouts, have collectively consumed as much energy and thought as the "Origin of Evil," the Thames Tunnel, and the Greek Particle, all put together. Not that this outlay of labour has been followed by results at all commensurate, or by advantages comparable in any way with those accruing to mankind from the above-mentioned memorable evidences of human enterprise. The Greek Particle has made many a bishop; it is possible to get through the Tunnel, when you have found it; and the search after the Origin of Evil has led many a sinner to consider of his ways. But what can we say for Gout? There is a book just published, fresh from the pen of Dr. Garrod; here is another, by Dr. Gairdner; and in no one thing do these two gentlemen agree! A patient of our's, who has worn himself to a shadow, and spent "heaps" in mines, came to us joyfully, the other day, with good news. "Sir," said we, "what is it?" "Doctor," he replied, "we have received a report from the Tydvil-Morgans, and they have hit on a lode *congenial to copper!*" *We* don't hit on a lode congenial to copper. We work the mine to death, but, in the language of the money market, there is nothing for the adventurers. Gout is, in fact, the pyramid of medicine. There are a great many labourers: a large mass of rubbish: and nothing inside but blighted hopes and vain phantoms of broughams in the air.

Is it a painful or a gratifying fact to dwell upon, that any medical bookseller can readily supply more than three hundred works on gout? Is Great Britain gouty to a man, or are publishers the most sanguine of human beings? Are medical men related to these gentlemen by the mother's side, or are books written for wagers? Do M. Ds. feel irresistibly impelled

to treat of chalk-stones, as geologists do to knock them about with hammers? or is there something superlatively attractive to the mind freshly imbued with the truths of pathological lore, in deposits of urea?

These are momentous questions. To authors, doubtless, publicity is its own reward. "On the occurrence of an attack of gout," reads a gentleman in the writer's study, a night or two back, "severe pain is felt in the articulations of the fingers and toes." "Articulations! Dear me, I never knew the toes could articulate." After that, none of us need despair of attaining to a high place in the estimation of the gouty ones.

What have we to show, then, for all this brain-work? Are we right in affirming a *materies morbi*? Can we analyze it—determine the conditions necessary for its development—the laws of its deposition—the means of antagonising it? Is our treatment less empirical than in Mrs. Steevens' time? "I have come to the conclusion," said an eminent author to us, "that there's nothing like Blair's pills. It's such stuff, you know. I go to a doctor, and he begins to muddle me. I ask him if colchicum won't do, and he dilates on the evils of quack medicine; and then the end of it all is, that he prescribes colchicum, and that's what Blair's pills are made of." Is this satire far from the truth? and have we nothing more to show for our work than the acetous extract, the wine of the seeds, and the bicarbonate of potash?

Not that *in limine* we wish to deal harshly with the work, whose title graces the head of this article. We must bear in mind that we are in presence of a great fact—a fourth edition—and, indeed, in a former Number of this Journal, we have, and with pleasure, bestowed our approval upon Dr. Gairdner's labours in the well-trodden field he is cultivating, with, apparently, such success. It is pleasant to find originality, where there is generally such sameness of matter and style; and safe rules for treatment, where mostly one is dazzled by hypothesis—even though the originality may be somewhat self-sufficient, and the treatment distinguished by nothing particular.

The motives that originally impelled Dr. Gairdner to write this work on gout are, we observe, persistent in his mind at the present time. It is but just that they be recorded, in order that posterity may not be altogether unmindful of the doctor's claims on its gratitude. After lamenting that certain observations and thoughts of his own are not reflected in the minds of his "colleagues," and recollecting that his own opinions have

undergone great change, Dr. Gairdner is driven to further inquiry; and, despite the diffidence which is the invariable attendant upon true merit, resolves to promulgate his opinions:—

“I shall be more readily excused for calling upon physicians to read another book, if the opinion I entertain of the great frequency of gout be correct. We are apt not to consider a man as gouty, unless he has suffered under a regular fit of the disease.”

We are in duty bound to deprecate this display of ruthless jocularity in a scientific work. The author is evidently twitting his “colleagues” in London, who, it is notorious, are utterly benighted, and, on the question of gout, altogether in a sad way. We can, however, safely speak up for the practitioners in Dublin, and are confident there is not one of them who is not thoroughly alive to the frequency of gout as a latent source of disease—to whom gout would not suggest itself in any case of dropsy, or of iritis, or of fluid in the pericardium, or of cerebral softening, angina, palsy, or cardiac dilatation—signs, in fact, pertaining to diseases constituting, in our experience, the staple of consulting practice.

Agreeing, then, with Dr. Gairdner as to the frequency of gout, we naturally take stock of the prevalent notions on its history and nature, and inquire wherein the views of the author are notably at variance with them.

If we were asked to name a disease essentially constitutional, both in origin and progress, or a disease having for nature a depraved condition of the nutrient fluids, we should unhesitatingly specify gout. That the blood is the seat of gout, is the prevalent opinion; and so strongly do we incline to it, that we go even further in our belief, that a correct and thorough knowledge of this disease is at the bottom of all sound pathology. Nor do its manifestations, essentially local as they are, invalidate this view; for they are invariably accompanied by indications of constitutional disorder. The constitutional disorder may be idiopathic, or induced; the first preceding deposition, either in joint or texture; the latter, consequent on such deposition. The primary disorder originates in depraved assimilation, having its seat in the secreting or excreting apparatus; by which either the nutrient gland-cells fail in abstracting from the blood the elements necessary for their own nutrition; or the excretory gland-cells perform imperfectly their office of separating certain complex substances from the blood, therein deposited as effete material. How far this paralysis of function may depend upon disturbance in the nerve-apparatus of organic life;—or whether, if such there be, it may not itself

be dependent on abnormal nutrition, it is, and always will be, impossible to decide. The first certain fact, however, is a failure or perversion of the assimilative functions. A disturbance ensues as a *necessity* in the chemical balance of organic life, resulting in changes altogether incompatible with conditions of healthy vitality. The state of the system, we think, may aptly be compared to what is manifest to the eye in the occurrence of a chemical decomposition. A fluid, perfectly devoid of colour, is brought into contact with a re-agent, and forthwith, however minute the quantity of this latter may be, a change is induced in the whole mass of fluid. Its composition, the arrangement of its atoms, is altogether modified. Its nature, its physical characters, its chemical properties, cease to be the same. Before the gouty diathesis, however, is fully established, nutrition is not altogether stayed, and excretion is sufficiently complementary. But the poison is cumulative, becoming more and more stored up by its own impairing influence on the functions. When in sufficient quantity, a true toxemia is induced. This toxemia is, to say the least, most intimately associated with the presence of urea, uric acid, and its salts. Whether the poison or poisons be attendant upon these compounds, or be the compounds themselves, the first two accumulate in the blood; and one of these latter, the urate of soda, eventually deposits in the tissues. Further, we know that gout is frequently traceable to the parent immediately, and that it instances the curious phenomenon of atavism; that it may be engendered by a *vie oisive* and free consumption of aliments rich in carbon and nitrogen, and averted, or at least mitigated, by abstinence and exercise.

We think there is no room for doubt but that the humero-vital doctrines are alone capable of reconciling these several phenomena. Can there be anything more analogous to the zymosis of a miasm and the formation of an eliminative pustule in an eruptive fever than (although not by fermentation) the accumulation of urea and the deposition of the soda salt? It is as well, therefore, that we should benefit by the matured views of Dr. Gairdner, as expounded in the present edition, and these are they:—

“Venous congestion, then, I consider the first condition essential to the formation of the gouty diathesis. It is no new observation; it may be found interspersed through the writings of all former authors. Even those who adopt explanations inconsistent with such a state of things, notwithstanding, admit it. This state of the blood was first clearly announced as the great cause of gout by Galen, whose opinions have continued to influence the minds of suc-

ceeding physicians, in a greater or less degree, to the present day. The truth of the fact being, I imagine, unquestionable, it will always continue to embarrass the doctrines of those who advocate opinions with which it is incompatible.

"But the great venous canals of the body, as well as the larger arterial vessels, are endowed with a resiliency which enables them to struggle well against the flood of returning blood. This fluid, then, is compressed between two opposing forces, that, namely, which is derived from the heart and arterial system, urging it forward on its course, and, on the other hand, the antagonistic resistance of the great veins leading to the right auricle. Under this compression I believe that the vessels give way, and a true hemorrhage is occasioned in the part affected. If the rupture take place in a minute capillary carrying the serous portion of the blood only, œdema is the consequence; but if the burst vessel be one carrying red blood, a true ecchymosis is formed.

"This view of a fit of gout may startle, from its novelty; but I am thoroughly convinced, from long observation of the disease, that I have given the true rationale. All its symptoms may be readily arranged under this explanation. Any other that I have ever heard of leads us into such difficulties, as to leave us only in greater doubt than before. It will surely be admitted that the capillary and nutrient vessels, distributed on the extreme and sentient fibrillæ of the nerves, are affected in the same manner as the larger venous trunks. I believe these distended capillary vessels are the real seat and cause of the painful phenomena of gout."

It will be gathered from the above that venous congestion is essential to the production of the gouty diathesis and to the fit itself. The direct inference is, that slight venous congestion induces the diathesis; intense venous congestion, a paroxysm. Between the two conditions are many intermediate states, readily accounting, it would seem, for the phenomena of this disease;—its hereditary character, its metastasis, its association with peculiar states of the urine, its affection for the joints. If the gouty diathesis is dependent on venous congestion, what does the venous congestion depend on? Certain sects, once on a time, affirmed that the world rested on an elephant, who was supported on a willing tortoise. "But," said the captious of the period, "what does the tortoise stand on?" We would delicately suggest that the faulty elimination, deduced as a consequence of the fit, is the cause of it. To affirm that a paroxysm of gout is due to the hyperemic condition of the viscera, is to reiterate the exploded view of Dr. Parry, to whose patients Dr. Prince recommended the tread-mill, and who presumed, that not only gout, but most diseases, consist in local determinations of blood.

Venous congestion must, in truth, itself be a result for which true physiology would at once seek for a cause. Arrest of the circulation in the veins must be owing to one or more of these influences—deficient resistance of vein-coat, want of power in the heart, obstruction between congested spot and the right heart. Now, in order to establish the gouty diathesis, is one of these causes indispensable? We think not. Indeed, we have but to open the head of a person dead with gout in the brain, and palsy consequent upon the establishment of the gouty diathesis, to find the utter absence of venous congestion, and the presence of white softening. Indeed, the ganglionic masses are often found in a state of semi-deliqescence from interstitial atrophy, a condition demonstrative of a nearly total deprivation of blood from the cerebral pulp; a state of the system opposed to plethora, and a condition of the brain altogether different from that observed in sanguineous apoplexy, where a laceration of brain-fibres ensues as a consequence of venous congestion. On this congestion-theory, gout should reasonably be expected to develop itself preferentially in those structures, and organs exhibiting them, which are dependent for their integrity upon that of a very complex venous system. Such, for example, are the liver and the kidney; but we are not aware that obstruction in their portal systems results in gout, or that this disease manifests any predilection for either of these glands.

Is, then, this supposed venous congestion due to want of resilience in the vein-coat? Such deficiency must be ascribed to a withdrawal or impairment of the nerve force to which the coat itself owes its physico-vital properties. And, similarly, where the heart itself is deficient in power, the *origo mali* must, in like manner, have its seat in the nerve-centre or in the conducting element. If, again, we assume an obstruction between the seat of gout and the right heart, such obstruction—its equivalent, venous congestion—cannot but be regarded as itself dependent on an exciting cause.

In truth, venous congestion is only one of the signs of that condition of the body of which gout is the exponent. Venous congestion occurs in a vast number of diseases and states of the system; at hap-hazard, for example, in inflammation: in purpura: in bronchitis: in cerebral hemorrhage: in pregnancy: in varix: in gangrene. Frequently these conditions, or some of them, may be immediately deduced from venous congestion. It would be far more philosophical to conclude this venous congestion to be *the* cause of these several diseases, than that it is the cause of the gouty diathesis, for in this latter it often happens there is an utter absence of congestion of any kind.

What, now, presses in favour of the humoral view of gout? *A priori*, it is as reasonable to suppose a *materies*, as a *defectio* or an *obturamentum*; a special poison, acting on the whole constitution—on all the systems at once—as a faulty state of the venous system always preceding hyperemia. A poison admits the phenomenon of metastasis; not so venous congestion. A poison, syphilis, for example, can be transmitted from the parent. Can venous congestion? A poison may be eliminated; and, if a poison there be, its effects may be mitigated by the elimination. No such evidence testifies in favour of venous congestion. Why can a gouty stomach bear a pint of brandy? Surely not because it is congested. It is a fair hypothesis to allege that the mucous membrane of the pharynx and the general cutaneous surface, throw off a poison in scarlet fever. It is sound induction, when we find the urine albuminous, and the kidney inflamed, after scarlet fever, to affirm that this organ has been endeavouring to throw off this poison, and has been damaged in the attempt. But we cannot get at the poison, nor do we know anything of its real nature. Yet it is fair to assume its existence. How much more, then, are we not justified in believing gout to be due to a poison, when in this disease we not only observe similar facts warranting similar inductions, but are able to get at the poison itself; to isolate it; to note its invariable chemical identity, and its constant attendance upon the disease; to arrest it in the circulating fluid, and at any point of the circulation; to mark its deposition as a vital precipitate, wherever and whenever the juices are subject to its influence?

From what has preceded, our readers will not be surprised to hear that Dr. Gairdner denies altogether the existence of an uric acid diathesis. Noticing the discovery of Dr. Garrod, that uric acid is found in the blood; that it had previously been detected in the joints by Wollaston; and by Landerer as a concretion between the coats of the aorta; Dr. Gairdner goes on to say:—

“One of the most remarkable results of arthritic indigestion is the presence of acid in nearly all the excretions of the body. This has of late years been called the uric-acid diathesis. The adoption of such a term would imply a conclusion to which I am by no means prepared to assent. By a diathesis, I understand a condition of the constitution, and not a simple affection of certain fluids of the body. Undoubtedly this acid condition is very general.

“But, admitting that these accumulated facts are sufficient to justify us in the use of the expression to which I have adverted, it could not, by any means, be called the uric-acid diathesis, for it ap-



pears certain that various acids make their appearance in the secretions. Berthollet supposed that the acid of the sweat was the phosphoric; Berzelius and Anselmino showed it to be the lactic; Thenard found acetic. No one, too, as far as I know, has ever found uric acid in the stomach; but there seems little doubt of the presence of the hydrochloric and lactic, and the acetic, in cases of dyspepsia. This idea of a uric-acid diathesis as the cause of gout, meets with another and a fatal difficulty, from the fact that urates in the urine are common to gout with many other diseases. Ordinary indigestion, from whatever cause, or a slight and ephemeral fever, will cause this appearance. Dr. Prout has indeed here established a solid and useful distinction between that deposit of urates which is occasional and transient, and that which is more durable. 'The lithate of ammonia in the urine is one of the most common attendants of slight dyspepsia from errors of diet.' But the same substance makes its appearance in the urine in a less transient manner, in the course of severe diseases not of a local nature, 'when no food has been taken into the stomach, and when, therefore, its formation can only be attributed to secondary mal-assimilation of the albuminous contents of the blood, and albuminous tissues.' I have seen, too, and I am sure Dr. Prout has seen, very durable deposits of the urate of ammonia in cases, such as lepra or psoriasis, where no great constitutional disturbance existed, where no unusual disintegration of the tissues was going forward, and where no interruption to ordinary wholesome nourishment was suspected. But, in order to establish the presence of uric acid in the circulating fluids as the cause of gout, it would be necessary to prove that it is never absent."

If we apprehend the author correctly, he objects to the term diathesis, because the uric is not the only concomitant acid of that state of body of which it is characteristic. That is to say, a person whose urine deposits lithic acid and its salts both in excess, and during considerable and frequently-recurring periods, manifests no lithic-acid diathesis, for the reason that other acids are simultaneously excreted by the emunctories; phosphoric, by the skin-glands; muriatic and acetic, by the gastric follicles. Surely, this is no more than to say, that a disturbance in the balance of the excretory system is impossible, as long as any one of the minor functions is properly performed. Supposing phosphoric acid were an abnormal product, or were produced in abnormal quantities, or, so to speak, at a wrong point of the system, we should not hesitate to believe in the phosphatic diathesis. By diathesis we suppose a tendency, and when the tendency is confirmed, we call the result a cachexia. In the hemorrhagic diathesis, there is a tendency to bleeding; in the oxalic diathesis, a tendency to the formation

of insoluble salts of the acid; and such diathesis may be temporary, or irremediable, and, as it were, an element in the vital equation. We see, to the belief in an uric-acid diathesis, none of the "fatal difficulty" that oppresses Dr. Gairdner. Unquestionably, urates in the urine are common to many other diseases than gout; but a diathesis is not negatived by the manifestation of its *characteristic* incidentally in the course of other diseases, not resulting from that diathesis. For example, it is by no means unusual to detect sugar in the urine after eating starch, or in the urine of patients attacked by pneumonia, or by pharyngitic epidemics with exudations of plastic lymph. Is it, therefore, to be inferred that a diabetic patient presents no saccharine diathesis, because he eats starch one day, or has a solidified lung, or diphtheria, on the next? Everyone believes in a tubercular diathesis. But, according to this view, a patient dying of phthisis pulmonalis would not present it, should tubercles deposit in the omentum—nor another, the hemorrhagic diathesis in purpura, for the reason that petechiæ form in continued fever.

The fact seems to be that, in all cases, long previous to an attack of gout, the system is in a state of active waste. The result of this waste is uric acid and its compounds, the decay affecting the fibrinous and azotised tissues generally; or that, previous to conversion into texture, the nutritive element is thrown out without subserving nutrition and repair. However this may be—whether the nutritive matters be supplied in excess, or the selecting power of the textures themselves be deficient—a *degrading impetus* is sustained, and organised material is converted into uric acid. The discovery of Dr. Garrod enables us to conclude that the acid in question is not formed by the kidney, but in the laboratory of the system at large. How long the casting off of this poison may go on, it is, of course, impossible to say. The limit to its elimination is dependent, no doubt, on the inherent powers of resistance to its influence which pertain to the individual, and to the ability of the several glands to cast it off. In some persons, profuse acid secretions of the skin—in others, copious deposits in the urine, indicate the attempt to throw off the burden, and for a time the effort is successful. But by degrees, from contamination of the nerve apparatus with the poison, the innervation of the glands is modified, and partially annulled. Antagonism then ceases; and deposition forthwith takes place in some portion of the system, offering the least resistance to its action. As a rule, the joints are the most attacked; but they are not the only localities selected. Dr. Garrod goes so far as

to assign to inflammation the power of inducing the deposit at the inflamed spot; but herein he seems to refuse the very advantages the humoral view bestows of educating the complete history of a paroxysm; and to our thinking it is the truest, as well as the most reasonable view, to look upon all inflammation as a consequence rather than a cause.

The uric-acid diathesis we do not intend to assert is peculiar to gout, but it is more peculiar to gout than to any other known disease. We agree with Dr. Gairdner that many states of the system are characterized by its presence. Where such is the case, gout, we should infer, was on the road; and where gout itself is present, we believe the uric-acid diathesis to have gone before, and to have been essential to it. If this be true, an attack will be best averted, and a paroxysm mitigated, by so influencing the nervous system that it may, by resistance to the influence of the poison, be sustained in its attempts to further the eliminative process.

We have been led to this conclusion by frequent observation, in common with Dr. Gairdner, and both in hospital and private practice, of the simultaneous deposition of phosphates and urates, and the alternation of the two sediments. We have a shrewd suspicion one deposit is to a great extent vicarious of the other; and that the causes bringing about the degradation of the azotised principles have a like influence on the tissues abounding in phosphates. It would be interesting to determine the exact relation these two deposits bear the one to the other. Does a deposit of urates invariably precede one of phosphates? or does the waste of nerve texture, denoted by the latter, bring about the excess of urates, and become in fact, the cause of that deficient vital resistance ending in the waste we above have alluded to?

Endeavouring to demolish the uric-acid hypothesis, Dr. Gairdner calls for an explanation of the fact, with which he believes it irreconcilable, "that the first attacks of gout, when the so-called uric-acid diathesis is least developed, are generally by far the most painful," and then continues:—

"But the inadequacy of Dr. Garrod's hypothesis for the explanation of the attacks of regular gout becomes hopeless deficiency, when we advert to the phenomena of the atonic disease. Could I admit his dictum, that an excess of urates may lurk in the blood without notice of their presence, my difficulty might be less, though by no means removed. But I entirely repudiate this unfounded opinion. It is offered to me without proof, in the shape of an assertion. I see in it only an assumption necessary to complete an erroneous theory. I am quite satisfied, from long and *painful* ob-

servation, that the presence of an excess of urates in the blood is always attended with more or less distress; that the great majority of cases of suppressed atonic gout present not a trace, not a sign of uric-acid diathesis, either in the urine or the blood; that this makes the great difficulty of their treatment, and that their cure is coincident with the appearance of this supposed cause."

The explanation of the extremely painful character of first attacks of gout, we think, is easily arrived at. Pain is the result of a morbid impression on sensitive nerves, which eventually become inured to the poison. The unnatural stimulus acting upon them, the nerve-tube and its contents undergo an actual chemical change. The nerve is partially disintegrated, and perfect *polarization* takes place no more. After successive vitiations of the nerve-texture, there is no longer that violation of those previous and integral conditions, the primitive disturbance in which manifests itself in pain of an acuter kind than that which results at a later period.

That this view is correct, we think, is demonstrated by the gradual softening and degeneration of nerve-substance, whether in fibre or in ganglion, that always ensues upon the long persistence of gout. It may be urged that gouty persons do, on the contrary, possess perceptions as clear, and attributes of nerve-power as subtle, as those of other men. But such an objection would originate in the omission to take into consideration the portion of the brain-mass affected; and to invalidate the above view, it would be necessary to show either that the poison settles by preference upon the hemispheres, or that the hemispheres were of collectively greater mass than the centres of motion, of sensation, and of co-ordinating power.

We are not inclined to go to the same length in condemnation of Dr. Garrod's hypothesis as Dr. Gairdner. We see no violation of analogy in supposing that urates may lurk in the system. Will Dr. Gairdner maintain that the agents producing diseases not gouty may not remain latent, giving no outward and visible sign? We would, for example, instance ague and epilepsy, not to speak of the dormant state of the cancer-cell, and the incubation-fever periods. In ague at regular, and in epilepsy at irregular intervals, a manifestation occurs of the presence of a disease which, meanwhile, might be supposed to have no existence. Are the diseases and the elements of them absent between the fits? present only when recognizable by the senses? Is the patient suffering from ague only when he is shivering, and another afflicted with epilepsy only when foaming at the mouth? The facts point the other way. The diseases, perhaps the causes producing them, cer-

tainly the agencies perpetuating them, are present, though all signs and symptoms are absent. The body is epileptic during the interval between the cessation of a fit and the advent of the next; and so, in like manner, is it suffering from ague, though the shivers are not "on." And, in fact, Dr. Gairdner allows this to be so; for in that portion of his work which has the most originality—that, namely, where he classifies gout—he affirms the persistence of the disease during the intervals between the attacks.

Cullen recognizes four so-called species—varieties they would be better termed—of gout. They are the varieties recognized by Good, and admitted by Scudamore and Macintosh. Such are the regular, the atonic, the misplaced, and the retrocedent. Of these, Dr. Gairdner admits only the regular and the atonic; and, in describing the regular variety, divides it into stages, each of which may be marked by successive paroxysms or fits. This is, in fact, to acknowledge that the gout is present in the intervals, when there is an absence of all deposition or inflammatory action, and when no trace of gout is to be observed.

The author, in dilating on his own classification, becomes graphic, and revels in symptoms, as Abernethy did in a poultice. But the symptoms will hardly be a source of misinterpretation to any physician of the present day. They are simply those that may be educed by a consideration of the tissues affected, and the perversion of those functions to the performance of which they are subservient.

"The earliest sign of an approaching fit of the gout, to which my attention has been drawn, has been a dull pain in the left side of the chest, accompanied by an inability to lie on that side, and sometimes by fluttering, irregularity, or intermission in the action of the heart. These symptoms often continue for a great length of time, without any perceptible increase. Patients not apt to take alarm about themselves, frequently suffer them in silence, and even forget them."

This subsidence or "sinking" in the chest is a frequent premonitory symptom, accompanied by intermittent pulse. In our experience, however, most patients anticipate an attack from disturbance connected with the digestive organs. We have a patient under treatment, at this moment, in whom an attack is always preceded by distressing flatulence. Three or four days after the advent of this symptom, the gout "flies to one of the joints." As soon as the inflammatory acme is attained, all gastric symptoms disappear; but while the dys-

pepsia lasts, our friend is obliged to lock himself up, as the constant noise resembles that of a park of artillery announcing the coming of a conqueror. This patient has never presented a symptom indicative of functional cardiac disturbance.

The following accords with general experience:—

“The local manifestations in the joints are usually looked to as the most characteristic signs of impending gout.

“Yet, certainly, persons who are threatened with a fit of gout, do very often experience great tenderness of the feet in walking, weakness and pain of the ankles and wrists, pain of the ischia in sitting, and frequently have swellings of the articulations of the fingers and toes.”

We have in our mind a gentleman in whom the joints are exquisitely susceptible. An attack of gout may at any time be brought on in this unfortunate patient, by treading on his toes, or squeezing a finger. Experience has taught him some severe lessons in this particular; and he rigidly taboos all shaking of hands, and other such like demonstrations of friendship. When he sees any one about to shake hands with him, he puts his arm out at full length, and points the little finger. If he were not known as the goutiest, he would be certainly thought the oddest of men.

One anomalous symptom referred to by Dr. Gairdner is pain in the tonsils (p. 11). This, we should think, is very rare, nor have we ever met with such a case; but, on the other hand, many attacks of gout we have found declaring themselves by giddiness, and hesitation of speech. It is a question whether the several symptoms may not each be taken more or less to indicate the kind of gout that is coming on. Thus dyspepsia, irritability of the bladder, &c., we think, may be looked upon as denoting that the attack will expend itself mainly upon the mucous membranes; tenderness in the joints, that the synovial and serous membranes will be principally visited; pain in the heart, and irregular action of it, that the muscular system will be most punished. We should have been glad to note the results of Dr. Gairdner's observations upon the renal secretion, for we are satisfied much remains to be done in this department of physical medicine. The urine, our author justly observes, is mostly, at the onset, scanty and high-coloured; and, at the close of the attack, throws down a deposit, which, usually supposed to be urates, may consist in great portion of mixed phosphates. Not unfrequently, however, we have seen the urine voided in large quantities. In many patients the symptoms are, it is true, those of pyrexia; but, in some others, it

would seem as though a temporary hemotrophy of the kidney were brought about by the increased efforts at elimination. Not unfrequently, indeed, the urine is diabetic, with *sp. gr.* higher than 1020, a condition rarely met with unaccompanied by diuresis. We do not know why the presence of oxalate of lime, and the alternation of that salt with lithates, are not alluded to in the work, or why no information should have been afforded on the very frequent excess and probable nature of those obscure principles of the urine, which display themselves by the modifications in colour produced by the addition of nitric acid.

That many, if not most, of the terminations of gout are to be referred to annihilation of function in the kidney, is generally conceded at the present day, when so much light has been thrown upon the pathology of this organ. We should have expected that a fourth edition of a work on gout, published at this time, would have noticed the views and researches of Frerichs, of Todd, and of George Johnson. What would be Dr. Gairdner's prognosis with an abundance of oil-casts? Is the microscope of any practical value in cases where the diagnosis is uncertain? Is there any special nephritic affection attendant upon gout? Does the elimination, set up mainly through the kidney, induce any peculiar changes in the uriniferous tubes and their lining epithelium? These are, perhaps, the most important questions connected with practical physic; and, in a fifth edition, we trust some notice will be taken of the advance that has been made in this department of pathology, since the publication of Dr. Johnson's celebrated paper in the *Medico-Chirurgical Transactions*.

We are inclined to believe that that peculiar modification of chronic nephritis, or, to speak more correctly, the contracted granular kidney, may not only be the invariable concomitant of gout, but be, at times, the only result of the diathesis. We do not doubt but that those morbid atrophic lesions, characteristic of the influence of gout, are to be observed even more frequently than those changes effected by the *materies morbi* in the joints, the sheaths of tendons, the bursæ, and the synovial membranes. Most kidneys of persons suffering from gout would, we think, could they be examined during life, present traces of the shrivelled granular appearance. Waste is the rule in gout. In the kidney, this waste takes place at the expense of the secreting structure—the cortical portion presenting, on a cut section, the granular appearance identical with that of the surface, from which the capsule may be torn with great facility. The microscopic appearances are, under these circumstances,

peculiar. It is common to find the tubes altogether free from epithelium. Others of these tubes are semi-denuded, shrivelled, and folded; and not unfrequently a degeneration of the epithelial cells into fat is found to have occurred. Simultaneously with these conditions, an obliteration of many of the blood-vessels takes place—a state of things accounting for the wasting of the organ. It is by no means unusual for tubes to disappear altogether, and to be replaced by cysts, a phenomenon accounted for by Dr. Johnson, by the assumption that the tubes may become altogether transparent from the loss of their epithelial lining.

How may these pathological changes be made subservient to diagnosis? A patient about the middle period of life is, say, free from dropsy. There are no pathognomonic signs of gout. There may be no hereditary taint, nor even a history of sedentary life, or free indulgence in "alcoholic food;" nor is there any disturbance in cardiac function. But if the urine be allowed to stand for a few hours, the secret comes out. Examine the sediment. There are no lithates, no uncombined lithic acid; but there is an amorphous substance—the disintegrated epithelium of the kidney-tubes—part of which is aggregated together in the form of tubes, and called by Johnson, granular casts. With this condition, there may be very little or no albumen. The quantity of the urine may be normal—certainly not diminished—and its sp. gr. even remarkably low.

Such a case, therefore, as that narrated at pp. 80–84, is to the physician of little value. The case is one of atonic gout. During the progress of the disease, partial dropsy comes on. The urine is tested several times, but presents no evidence of albumen. The *post-mortem* examination expends itself mainly upon the circulatory apparatus; and the kidneys are thus dismissed, "the kidneys were healthy." We doubt that the kidneys were healthy, and should have suspected them to be in a state of granular degeneration, even without the dropsy. The absence of albumen is often noticeable. One day the urine may be albuminous; the next, free from all traces of the principle. In the last stage of granular kidney, we have had frequent occasion to observe an utter absence of albumen; and, under similar circumstances, we may safely conclude that the atrophy of the kidney is very far advanced.

Dr. Gairdner's treatment is, we hope from the success that has attended it, decidedly antiquated. We see no mention of applications of spirit of wine, or the oil of horse-chesnuts, to the joints. Alkalies and earthy carbonates, as might be expected from Dr. Gairdner's denial of the uric-acid diathesis, are



pronounced "wholly inefficacious," p. 323; and the carbonate of lithia is not exempted from this disparagement. We humbly trust that such an authority will by-and-by see good reason, at all events for abstaining from those blood-lettings which at present have his cordial approval; and will believe that the success of a work need not be diminished by its author posting himself up in the science of his subject, even though the knowledge thus added to the text should necessitate the curtailing of some very scholarly and agreeable writing.

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*On the Signs and Diseases of Pregnancy.* By THOMAS TANNER, M. D., F. L. S., &c. London: Renshaw. 1860. 8vo. pp. 504.

DOCTOR TANNER does not come before the profession for the first time. He has produced a variety of medical works, some of which have passed through more than one edition. Thus, there are his "Practice of Medicine," his "Treatise on Diseases of Infancy and Childhood," his "Manual of Clinical Medicine," his "Memoranda on Poisons," and his "Select Medical Formulæ."

The Doctor must have commenced his literary career previous to 1853, inasmuch as the *second* edition of "Select Medical Formulæ" reprinted from his "Manual of the Practice of Medicine," bears the date of 1854. The volume of "Practice of Medicine"—we have not the original edition by us—may, therefore, be presumed to have made its first appearance in 1852 or 1853, perhaps earlier. Now, as we are informed in the Preface of the volume before us, that its author commenced practice "some thirteen years ago," or in 1847, it follows that his first, and, according to its title, we presume his chief work, was given to the world at the expiration of the fifth or sixth year of his practice. Strange as this may appear, still more strange is the fact, that during the period when he was gathering materials from his practical experience, to write a *practical* treatise upon such an important branch of our profession as *practice* of medicine, our author tells us his "horæ subsecivæ were not altogether few or far between." So that the absence of patients, by no means an uncommon state of affairs in the first portion of a physician's career, was no bar whatever to the production of a volume, teaching us how to discover their ailments, and to assist nature for their removal!

Not satisfied with having produced all the publications above enumerated, from time to time, in the latter half of a

period of thirteen years, Dr. Tanner now presents us with an *opus magnum*. This our readers may presume to contain the result of the full period of, at least, six years' experience, when it is hoped his "*horæ subsecivæ*" were not so numerous. And here we may observe that the "*horæ*," which he devoted to the revision of the proofs of the volume now under consideration, might probably, with more propriety, be termed "*subsecivæ*" than those which he spent—not having anything else to do—perusing the writings of others; since the proper signification of "*subsecivus*" is, stolen, spared, or borrowed from *other business*.

Be this as it may, our author grew a

"Midnight student o'er the dreams of sages."

He conceived he could not occupy his spare-time better than by consulting "many dusty old volumes," with a view towards mental exercise; and having stored his mind with the ideas "of those great ones of the past, who can now only speak to us by their writings," he here acts as the medium of communication between the dead and the living.

Taking, therefore, all the circumstances above set forth into consideration, it must be confessed, we are not in the least surprised at the author's presentiment, that "the references which are made to old authors (he might, also, have added modern) may, perhaps, appear more numerous than is desirable."

This is truly the age of book-making; and amongst no class is the *cacoethes scribendi* a more prevailing epidemic than amongst ourselves. We were fully conscious that if an old doctor, now-a-days, spends his "*horæ subsecivæ*" at ever so quiet a watering-place, he expects us, subsequently, to peruse his "physician's holiday;" but we never contemplated the probability of our having to wade through a set of annotations, collected during the course of elementary study to which a young physician submitted himself, for the purpose of acquiring sufficient knowledge to enable him to *commence* the business of his profession.

Such is the nature of the volume before us—a compilation from beginning to end, with here and there a few running commentaries of the author's own, as to whether his experience coincides, or the contrary, with the experiences set down. In other words, this book may be looked upon in the light of a "grinder's" note-book, relative to the signs and diseases of pregnancy; and, in this respect, it partakes of the character of all Dr. Tanner's publications. Therefore, in taking a dip here

and there through its pages, we shall observe, not so much upon the author's own experience as upon that of others.

The great advantages of auscultation in obstetric medicine are now placed beyond controversy; but there was a period when its practice was scouted by more than one eminent member of the profession. Thus the author mentions Hamilton, with whose views on this subject we are all acquainted; also Lyall, who, in 1826, wrote as follows:—

“Of the utility or uselessness of auscultation in discovering pregnancy, we have had no experience. It is said that the operation may be performed either by applying the ear to the different parts of the abdomen, or by using the stethoscope of Laennec. Reasoning *a priori*, we anticipate little advantage from such examination.”

This language, written in 1826, does not excite our surprise, and may be excused; but it is a subject of wonder that in the present day, notwithstanding the overpowering testimony of, we may say, all the profession, an individual can be found, who gives utterance, not only to ideas similar to the above, but whose convictions on the use of the stethoscope to determine pregnancy (if we may call them convictions), he has set forth in the following manner:—

“Success in detecting the fœtal heart depends, in a great measure, upon circumstances; whether or not the auscultator considers that *he ought* to detect them.”

And he continues:—

“The whole system of fœtal auscultation originated soon after the dawn of general auscultation, when men's minds were excited by the love of novelty, and warped by many erroneous impressions and mistaken modes of thinking, and has since been mainly upheld by authority.”

However excusable such language might have been in the days of Lyall and Hamilton, it is now unpardonable. “I cannot but think,” the author observes, in allusion to this gentleman's writings, “his papers would never have been written, had he only taken the trouble to visit the wards of our lying-in hospitals before sitting down to his desk.” We say, however, that to this individual should be applied the gross charge that he has applied to others, viz., “that it depends in a great measure upon circumstances, whether or not the auscultator considers that *he ought* to detect” the sounds of the fœtal heart. And that the mendacity (for it is nothing less), which he lays

at the doors of others, he would be guilty of himself, by pretending he *did not* hear sounds which *he did* hear. Even supposing Dr. Francis Adams, from some defect of his auditory apparatus, is rendered incapable of detecting the sounds of the foetal heart, surely he is not warranted, on that account, to ignore the assertions of those who say they do hear them. However remarkable Dr. Francis Adams, "the learned translator of the works of Paulus Ægineta," may be for his classical learning; we cannot, after this, acknowledge him as a practical physician.

Dr. F. Adams' opinions appeared, it seems, in some Numbers of the Medical Times and Gazette, during 1859-60—opinions which must have created some surprise, and no little amusement. Strange to say, while engaged on the present observations, we have before us an article in the same Journal of as recent a date as the 5th of the past month, which bears upon the subject in question. This novel article would afford us considerable mirth, but for the regret that one professing to be of our calling should give utterance to such trash, exhibiting his ignorance and folly: and that a respectable Journal should prostitute its pages by the admission of such garbage. The thing is signed T. C. D.; but, for the honour of our University, we trust the initials do not signify Trinity College, Dublin. The following is a copy of the document, which we think worthy to be placed in antagonism with Dr. Adams' opinions on obstetric auscultation:—

"I ponder'd by the bed-side,  
I walked about the room;  
The stillness of my patient  
Fill'd my mind with deepest gloom.  
*The os was firm and rigid,*  
*Not a fibre of it stirr'd;*  
And the beating of my own heart  
Was all the sound I heard.

"I sat me by her *right side*,  
And watch'd her pale, pale cheek;  
But as it grew still paler,  
I did not dare to speak.  
I watch'd her for a moment,  
Then asked her for a word;  
But the beating of my own heart  
Was all the sound I heard.

“ It came not; no, it came not;  
 The night was passing on,  
 I knew not but my patient,  
 And *her fœtus* might be gone.  
 To the precept of my teacher  
 A *happy thought* refer’d,  
 When he told me most profoundly  
 What sounds were often heard.

“ The stethoscope—then gently  
 I press’d it to her side,  
 As though *she’d been an angel*,  
 Or *were to be my bride* ! !  
 I press’d it nearer, nearer, nearer.  
 A sound! ’tis clearer, clearer, clearer.  
 I did not speak; no, I did not speak a word,  
 But *the beating of the fœtal heart it was the sound I heard.*”

Our only excuse for quoting the above is, that emanating, as it evidently does, from a person altogether unacquainted with obstetric medicine, it is, therefore, the more worthy of being brought forward to refute the opinions expressed by Dr. F. Adams, who is equally innocent of it.

Dr. Tanner mentions, parenthetically, the invaluable testimony afforded by auscultation during parturition,—in those tedious cases, for example, in which the fœtal heart can be heard to pulsate naturally during the early stage of labour, and, as the labour advances, to beat unnaturally; in other words, slower or quicker than previously. We fully concur as to the great value of auscultation during tedious parturition, as indicating the state of the child’s circulation, and consequently the time for interference; but we cannot quite agree with him as to the differential diagnoses he thus forms in such cases. He says:—

“ When in tedious and difficult labours we find the fœtal pulse becoming slower and slower, we may reasonably infer that *undue pressure is being exerted on the funis*, thereby causing the æration of the blood by the placenta to be imperfectly performed. When, on the contrary, the beats of the heart increase in rapidity, and especially when they likewise become irregular and intermitting, it is probable that the danger does not result from the pressure upon the umbilical cord, but *from compression of the brain, or from a source causing cerebral irritation.*”

Thus, Dr. Tanner conceives the effects of undue pressure on the funis can, in a measure, be distinguished from that ex-

erted on the brain of the foetus, by auscultation during parturition. In the former instance, the pulse becomes slower and slower; whereas, in the latter, from being accelerated, it becomes irregular or intermittent. We cannot go with him so far, even granting deficient aëration of the foetal blood *alone* originated from compression of the funis. It has been our experience, that the foetal pulsations become *very frequently* such as he describes them in the second instance (*viz.*, from a state of acceleration to that of irregularity and intermission, as indicating, according to his views, the condition of cerebral compression), when there was evidently no undue pressure exercised on the cerebrum; but when, on the contrary, the origin of the change of the foetal heart's action most indubitably arose from imperfect aëration. Impaction, more or less complete, or undue pressure on the cranium, can be recognised without the aid of the stethoscope to a certainty. His using the phrase, "or from a source of cerebral irritation," shows that his theory is not very sound, since the foetal blood becoming impure *is* a source of cerebral irritation.

Moreover, we do not admit that in the majority of instances of tedious labour the imperfect aëration of blood arises from compression of the funis. It very frequently happens that the contractions of the uterus, in these cases, even though they be inefficient, are, to a certain extent, continuous, perfect relaxation never occurring. The fibres thus maintain a sufficient degree of permanent spasm to arrest the current of blood from the curling arteries through the great placental sinus, and thence onwards through the sinuses of the uterus.

This origin of deficient aëration has also, on many occasions, been exemplified to us in rapid deliveries. In such cases the uterine action was not only powerful, but the contractions were so rapid, as to be without any decided intervals of complete relaxation. We do not consider pressure on the funis so frequent a cause of injury to the foetal circulation, as that to which we have just referred; indeed, we deem it more rare than suspected.

Our Continental brethren always aim at bearing the palm from us in acuteness of diagnosis; in the application of the stethoscope for the purpose of arriving at conclusions, they consider themselves peculiarly *au fait*. Here we are introduced to Doctor Frankenhauser, "who has been often able to say correctly, before birth, what was the sex of the child," by means of the stethoscope; and who, as the author informs us, "seems to infer that his rule *has never failed him*, provided it was put

to the test before the contraction of the uterus during labour had deranged the *natural frequency* of the foetal heart."

It is from the difference of the average frequency of the heart's pulsation in the male, as compared with that of the female foetus, that Frankenhauser draws his conclusions as to the sex of the unborn child, before labour has commenced. His theory is, that the mean number of beats of the foetal heart are more frequent in the female than the male; the mean natural frequency being 144 in the former, and 120 in the latter. However, when we come to consider that the greatest number of beats in the female is given as 138, that the pulsations run as high as 132 in the male; and that we have every variety of number from the mean in both to the extreme in both, we are not surprised that the author's "experiments seem, at least, to prove that Dr. Frankenhauser's observations will not apply to the intra-uterine children of this (London) metropolis." Neither, say we, to our own visceral population.

We must also notice another Continental refinement in obstetric diagnosis. Not satisfied with the aids of sight, touch, and hearing, Dr. Pollender gives a constant and unerring method for determining pregnancy by means of his nose! According to this authority, the vaginal mucus, during the pregnant state, has a "musty" smell, "something like that of the spermatic fluid or liquor amnii, and after examination *it cannot be mistaken for any other odour.*" It appears that Dr. Pollender never failed in a single instance to make a correct diagnosis by means of his olfactory test; and, "according to his experience, the odour is perceptible as early as the eighth day of gestation." To this, as a test of pregnancy, *if it be one* (for we have not, as yet, given our nose a *complete* obstetric education), we would apply the words Hamilton made use of with respect to auscultation, viz., "few cases can occur in actual practice where this test can be required." In the present state of our knowledge, it is quite unnecessary, and its publication is repugnant to our feelings. The idea to us, Islanders, of *smelling out pregnancy*, would be ludicrous, were it not revolting, so we shall allow Dr. Pollender to exercise his invention for his sole advantage. In a short time, it will not surprise us to hear that the sense of *taste* has been called into Continental requisition in obstetric diagnostics. It is unnecessary further to notice the contents of this volume.

As a note-book, it is well arranged, and gives fair evidence of the author's careful and copious reading.

Those who do not possess any of our valuable and comprehensive obstetric class-books, who have it not in their power to

consult Encyclopædias, or the standard original monographs upon the signs and diseases of pregnancy, may perhaps derive some benefit from the purchase of Dr. Tanner's compilation. It remains for us to say, that such a description of book was not required at the present moment; and we repeat, that it partakes too much of the nature (as all Dr. Tanner's works do) of a "cram-book." Had the author informed us that it was so in his preface, we might have dealt more leniently with him; but after so high-sounding a title, and such a bombastic introduction, we had a right to expect something better at his hands.

In conclusion, we are constrained to observe, that for one who has not yet had time to mature his experience, the author speaks much too confidently on points of diagnosis, and upon the utility of medical agents. With Dr. Tanner's opportunities, however, combined with his great habits of industry, we have no doubt but that, at some future period, he shall present us with an original and *practical* volume.

*An Expository Lexicon of the Terms, ancient and modern, in Medical and General Science, including a complete Medical and Medico-Legal Vocabulary.* By R. G. MAYNE, M. D. London: Churchill. 1853—1860. 8vo. pp. 1506.

*The Medical Vocabulary; containing a concise Explanation of the Terms used in Medicine and its accessory Sciences.* By ROBERT FOWLER, M. D., Edinburgh, &c. London: Renshaw. 1860. Small 8vo., pp. 366.

DR. MAYNE'S work, the result of many years' laborious research and judicious selection, has at length been completed by the appearance of the tenth part; and we are happy to say that the favourable anticipation we expressed at the time of the issue of the first fasciculus has been fully realized. The author has produced the fullest and most perfect medical lexicon in existence. His experience during the progress of the work, and the suggestions of literary friends, have enabled him to add a rather copious appendix, which, in a second edition, will, no doubt, be advantageously dispersed through the body of the work. With the present and concluding part, are given the preface to the volume, an explanation of the arrangement adopted, and an abridged dissertation on scientific nomenclature.

A couple of examples, taken quite at random, will best  
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convey an idea of the amount of information contained in this valuable work.

"Climacteric Disease.—*Pathol.* A term for that sudden change which occurs in many instances of advanced life, by which a patient falls off in flesh and strength before any loss of appetite or dyspeptic symptoms are complained of, and which appear to take place afterwards only by sympathy; it steals on so insensibly, that he is scarcely aware of his altered condition until his appetite fails, sleep leaves him, or what there is does not refresh him, his face becomes emaciated or bloated, his tongue white, his pulse more frequent; he has pains in his head and chest, his legs swell; but there is no deficiency in the quantity of the urine, nor any sensible failure in the action of the abdominal viscera, except that the bowels are more sluggish than they used to be. That this is disease, and altogether distinct from a natural or constitutional decay, is proved by the fact that recovery often takes place. The change referred to generally occurs about the fourth, or in the interval between the fourth and fifth climacteric periods. See *Climacteric*. Fr. syn. *mal climactérique*. Germ. syn. *Uebel klimacterisch*."

It will be seen that the above extract contains one or two faults in composition, which do not, however, interfere with the immediate apprehension of the author's meaning.

"Clāvus, i, m. (*Clau-do*, to shut or close; from its resemblance to the head of a nail, which this word also signifies).—*Pathol.* A corn, or horny round cutaneous formation, the effect of continued pressure from tight shoes, generally on the prominent parts of the toes; it has a hard, dry, central portion, which, on every renewal of pressure, acts on the acutely sensitive substance beneath, causing an intensity of pain which only those afflicted can appreciate; also, a very acute pain confined to a small part of the head, as if a nail were being driven into it; usually connected with *Hysteria*, and then called *Clavus Hystericus*: also applied to a *condyloma* of the uterus, to a tubercle on the white of the eye, and to a certain degree of prolapse of the iris through an opening in the cornea, it being pressed flat, like the head of a nail. Fr. anal. *clavus*, m.; *clou*, m.; *cor*, m. Germ. syn. *Hühnenauge*, f.; *Nagel*, m."

As a dictionary of unusual technical terms, we have, during the years Dr. Mayne's lexicon has been in progress, most fully tested it; and we have only to repeat the opinion we have already expressed, of its adequacy as a most valuable companion to all engaged in medical literature, whether as readers or as writers.

The Dissertation published with the last part, comprising critical remarks on scientific nomenclature, and which now becomes the introduction to the work, consists of two principal

divisions, the first relating to terms common to Medical and General Science, the second to those peculiar to Medical Science. The six sections of the first division treat, respectively, of,—1. Terms, from the Greek, ending in *ides* and *ide*; 2. of Terms formed from Greek derivatives, ending in *ideus*; 3. of terms from the Greek, ending in *adæ*, *idæ*, and *ides*; 4. of terms from the Greek, in *ōdēs*; 5. of Botanical terms, ending in *ia*, forming the titles of classes and orders in the Linnæan and Jussieuan systems; 6. of the Greek Aspirate, and the misplacement of its symbol H, in Latin compound Terms and their English Analogues. The six sections of the second division are, in like manner, devoted to—1. The pathological term *Anasarca*; 2. the anatomical term *Gastrocnemius*; 3. the anatomical term *Psalloides*; 4. The pathological terms *Emprosthotonos*, *Opisthotonos*, *Pleurothotonos*; 5. the anatomical term *Platysma myoides*; 6. the terms—1, *Acidum*, *Causticum*, etc; 2, *Ephemeræ*; 3, Narcotics, Antispasmodics, Tonics, etc; 4, Fibrine, Stearine, Salicine, &c.; 5, the diphthongs æ and œ, and their English translation; 6, the terminal *icus*; 7, the terminal *ismus*; 8, the terminal *igo*; 9, the term *Lambdoidalis*, &c.; 10, *Zygoma*.

For the author's valuable remarks on the foregoing subjects, we must refer to his work; we would merely observe that we agree with him in his opinion that the final *e* in such words as *fibrine*, *caseine*, &c., ought to be rejected. We also think with him that in "spherical," "demon," "equal," &c., we have abundant precedent for changing the diphthong in hæmorrhage and allied words into *e*—a mode of spelling we have always adopted in this Journal. We are of opinion, however, that it might be better to let *œ* stand in *fœtus*, *fœtid*, &c., than to write *fetus*, *fetid*, although the latter course might seem to be authorized by the analogy of *economy*, *penal*, &c. The author acknowledges that a difficulty in the way of this change would present itself, in such words as *gonorrhœa*, *diarrhœa*, &c.; but this difficulty he proposes to meet by adopting these latter terms as they stand, without translation, and underlining or italicising them in writing or printing, as the case might be. We think, however, that such a course would give unnecessary prominence to the words in question.

In fine, we have only to recommend most cordially Dr. Mayne's valuable contribution to our standard works of reference, and to congratulate him on the successful and fruitful termination of his "twenty-eight years" of well-spent labour.

The publication of the other little work, whose title we

have given above, we consider untimely and uncalled for : appearing just as Dr. Mayne's encyclopedian labours were brought to a conclusion, gives it the character of an attempt to rob him and his publisher of the reward justly their due for both work and enterprise, by offering to the profession a cheap substitute. We regret to see that it has been favourably noticed by some of our London weekly contemporaries: not for this reason alone, however, but because, after referring to several of its pages, we have come to the conclusion that in its compilation the author has shown a lamentable want of judgment, and an amount of ignorance not at all to be overlooked in the present advanced state of medical and general science. As an example of the former, we need only to give a single extract:

"ARSECOCKLE (Sc.) Formerly an inflamed pimple on the hips; in Scotland, at the present day, a similar one on any part of the body."

Now, we shall merely ask our readers, is this the style of information they would seek for in the pages of a medical vocabulary: would they not rather look for it in a dictionary of slang?

In the preface, Dr. Fowler states:—

"He has also sought to expunge all such phrases and words—coined by the forefathers of the profession—as appear to have become obsolete and disused, and not to have been recognised by the medical literature of the present century."

Now, notwithstanding this declaration, on looking through the pages, we find the following obsolete—nay, some of them ridiculous—terms explained:—Hapanismus, Hang-nail, Doloriferous, Knifesman, Lambative, Ropalic, Tractoration, Perplication, &c. The author's definition of one of these we must give, as another example of the information to be expected in the work:—

"KNIFESMAN. One addicted to operate in almost all cases."

In fine, to show we are not giving an unfair verdict in utterly condemning this volume, we shall select at random a few examples, in proof of the thoroughly unscientific and most generally incorrect definitions contained in it:—

"CEREBELLUM (L.) The little brain, or the hinder and lower part of the brain."

"DOLICHO-CEPHALÆ (Gr. *dolichos*, long; *kephalē*, head). Races of men whose cerebrum completely overlaps their cerebellum."

"HALF-CASTE. The offspring of mixed races."

"**HÆCTIC FEVER** (Gr. *ektēkō*, I pine away.) A slow, long-continued, or remittent fever which usually accompanies the termination of organic disease."

"**OLEO-RESIN.** A native compound of a terebinthinate oil and a resin, the proper juice of coniferous and other plants."

"**PORRIGO** (L. scurf in the head). A generic name of certain cutaneous diseases, principally characterized by an eruption of straw-coloured pustules, concreting into cellular scabs."

"**ROSEOLÆ.** A sort of hybrid exanthem midway in character between measles and scarletina [*sic*], but affording no protection against either."

And so we might go on quoting nearly the whole book; but we think we have said enough to show the worse than worthlessness of Dr. Fowler's Medical Vocabulary.

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*Anatomy of the Arteries of the Human Body, descriptive and Surgical; with the Descriptive Anatomy of the Heart.* By JOHN HATCH POWER, M. D., &c., and B. WILLS RICHARDSON, F. R. C. S. I., &c. Dublin: Fannin and Co. 1860. 12mo. pp. 374.

DUE allowance being made for the occasional variety or anomaly in the origin, course, or distribution of any of the arteries in the human body, it may be safely asserted that this system of vessels, as found in the men of the present day, differs in no respect from the arterial system of the men of former times; and therefore a descriptive account, provided it be accurate, which is applicable to this branch of human anatomy at one period, will require but to be stereotyped for the readers of later times;—it is only the same story over again. But, although the purely descriptive anatomy of the human body has become as it were so beaten a track, that it is in our days extremely rare to find in it anything novel, or which has not been previously observed, it will be admitted that the rapid growth and development of our modern surgical improvements is attributable, in no small degree, to the mode in which surgical has been, so to speak, engrafted upon anatomical instruction in our schools,—the principles of the former being impressed upon the mind of the student along with the dry and otherwise uninteresting details of the latter. In no department of anatomy is the practical application of this principle of so much importance as in that which relates to angiographia, including not only the enumeration and nomenclature of arterial branches almost innumerable, and ramusculi without end, but the relative and

the surgical bearings of the principal vessels of the several regions.

About the year 1839, the late Dr. Flood, a most able anatomist, and a highly popular and successful teacher of the science, published a small work for dissecting-room use, entitled, "The surgical Anatomy of the Arteries, and descriptive Anatomy of the Heart; together with the Physiology of the Circulation in Man and inferior Animals." This work enjoyed a high and well-deserved reputation. In time, however, the original edition became exhausted; and Dr. Power then undertook the preparation of a new work, which should embody the essential parts of Flood's treatise, and at the same time should contain some important alterations and additions; the latter, as we learn from the preface, had reference chiefly to the anatomy of the heart, of the *arteria innominata*, of the *subclavian* arteries, and the operations performed on these trunks—of the *axilla* and the *axillary* artery;—the operations on the *brachial* artery, and the cure of aneurism of this vessel by compression;—to the circulation of the blood in the liver and in the kidneys,—to the *iliac* arteries, and the operations upon these important vessels and their branches;—and to the *femoral* artery, &c. Dr. Power's work was published in the winter of 1849-50, but has been for some time out of print.

The book which is now before us appears to be something more than a new edition of that of which we have just spoken; in fact, many of the anatomical descriptions appear to have been altogether rewritten, and are given in a style which, while it lacks the conciseness which characterised the language of the former edition, is, in our opinion, better calculated to convey accurate notions of the subject-matter of which it treats,—inasmuch as the smooth and flowing kind of composition is more easily read than the close and succinct, while the facts are not the less easily remembered. In the course of describing the various operative procedures connected with the several arteries which have been submitted to deligation, the author has recorded some important facts in connexion with improvements in surgery which have taken place since the publication of the former edition of his work; for example, among the operations on the *femoral* artery, we find the account of Professor Porter's operation of tying this vessel for *femoral* aneurism, by making a *transverse* incision through the integuments, and so reaching the artery whilst enclosed within the funnel, and of course before it had given origin to the *profunda*. The operation has been performed several times in this city with success. Again, in the chapter on ligature of the *aorta*, we find the details of five

cases in which this formidable operation has been performed ; four cases are already well known ; the fifth occurred in the year 1856, and was operated on by Mr. South, who thus details the case :—

“The man was 30 years of age, and a hard drinker ; had had a strange uneasy sensation two months before his admission, and six weeks afterwards noticed a small, hard, pulsating swelling in his right groin, which grew rapidly, and when admitted was as big as a goose-egg. Soon suffered paroxysms of violent pain, and the leg became numb. Eleven days after, the aorta was tied without difficulty by a cut from the tip of the 10th rib to superior iliac spine. In the course of a few hours, first one, and subsequently the other limb became discoloured ; he was in profuse perspiration, and exceedingly restless ; died 42 hours afterwards. Examination showed false aneurism of right external iliac artery.”

By no means the least valuable feature of the present work is the circumstance of the descriptions being accompanied by upwards of sixty illustrations from the pencil of Mr Richardson. We have carefully examined the woodcuts, and consider them to possess several advantages over anything of the kind which has hitherto appeared in handbooks of anatomy ; we allude in particular to the fact that they are sufficiently small to enable a student to take in with ease at one *coup d'œil* the relative anatomy of an entire region of considerable extent, and yet there is none of them constructed on so minute a scale as to have necessitated the sacrifice of clearness in any part of the detail. Those who are practically conversant with the difficulties which attend the preparation of drawings of this kind can best appreciate the importance of the service which the author of the present work has received from the valuable and truly artistic co-operation of that able surgeon, Mr. Richardson. It might here be suggested, *en passant*, that the usefulness as well as the beauty of the illustrations would be much enhanced by having the vessels coloured ; any one possessed of a clear eye and a steady hand could easily colour them all in a short time,—the only materials required for the purpose being a little scarlet lake rubbed down with soft water for the arteries, and the same with the addition of a slight tint of prussian blue for the veins. We have just seen a copy coloured in this way, and can speak favourably of it.

The printing and the general finish of the work are highly creditable to the publishers, the type being clear and sharp, and the paper of a smooth and fair quality. We have with regret to notice some editorial errors, which, although not very serious, must still be considered as blemishes in an otherwise

irreproachable performance; we allude to such errors in phraseology as the "Operation for Lithotomy,"—a phrase which is placed at the opening of a chapter, as well as at the head of four consecutive pages, and may thus tend to lead students into error; next, to the misspelling of proper names,—that of M. Desault, as often as it occurs, having been spelled with double S; and, lastly, the violence which has been done to several words by the unskilful manner in which the printer has been allowed to deal with them in many instances, where the mechanical exigencies of the case requires a word to be cut into two parts, for example at the end of a line, thus:—des-cribe, hemorr-hage, Eus-tachian, anas-tomose, corres-pond, &c.

The greater number of the illustrations, the author informs us in the preface, have been executed by Mr. Oldham, of this city; and the remainder by Messrs. Butterworth and Heath, of London.

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*The Composition of the Urine, in Health and Disease, and under the Action of Remedies.* By EDMUND A. PARKES, M. D., Fellow of the Royal College of Physicians; Professor of Hygiene in the Army Medical School, &c., &c. London: Churchill. 1860. 8vo. pp. 404.

THIS excellent book fills a *lacuna* in medical literature, and supplies to the student and experimenter an accurate view of the present state of our knowledge of the important subject of which it treats. It is the only work, with which we are acquainted, that enables the student to ascertain the amount of labour bestowed already on the investigation of the urine, in health and disease, and to refer to the sources, where he can read for himself the results of the researches of the many explorers in this field of medical science.

It is divided into two books: *The Urine in Health*, and *The Urine in Disease*. The plan of both books is similar,—the one containing an introduction describing the normal constituents of the urine, and the other an introduction describing its abnormal constituents and sediments. In addition to the introduction, the first book consists of two chapters,—one on the variations of the urine during health from physiological conditions, and the other on its variations from the use of medicinal agents. In like manner, the second book contains, besides its introduction, three chapters, on the urine in acute and chronic disease (not renal), and on the urine in renal disease.

These chapters differ much in value and importance, some, as the chapter on chronic (non-renal) disease, being of the highest interest; while others, as that on the variations of the urine in health, from the use of medicinal agents, are admitted to be unsatisfactory by the author himself, who says:—

“It must be admitted that at present the experiments on no single medicine have been made sufficiently often; and there are few statements in the following pages [chap. ii.] which can be considered as proved, in the true scientific sense of the word.”

Professor Parkes is well known as an original thinker and observer on the subject on which he writes, and consequently all his statements are worthy of more attention than those of any mere compiler of other men's labours would be. For this reason, and also because we believe that this book will be much read and studied, particularly by candidates for Assistant-surgeons, who form a highly-educated class of our medical students, we shall enter somewhat into detail in our review of some of the statements made by the author, who, we are sure, will take in equally good part our criticisms, whether we agree with him or differ from him—

“*Amicus Plato, sed magis amica Veritas.*”

The most important portion of the whole book is, undoubtedly, the first section of the introduction to Book I., which contains a summary of the mean quantities of the normal constituents of healthy urine.

The constituents considered as normal are—

1. Water.
2. Urea.
3. Creatine, and creatinine.
4. Urine pigment.
5. Extractives.
6. Mucus.
7. Uric acid.
8. Hippuric acid.
9. Oxalic acid.
10. Formic acid.
11. Silicic acid, &c., &c.
12. Sulphuric acid.
13. Phosphoric acid.
14. Chlorine.
15. Lactic acid.
16. Ammonia.
17. Potash, soda, lime, magnesia.
18. Iron.



Some of these constituents are of much greater importance than others, and we shall confine our comments to these exclusively.

One or two preliminary observations are necessary on the method followed by the author to obtain his means.

1. He rejects (properly, as we think) all observations not referred to 24 hours as the unit of time.
2. He adopts, wherever it is practicable, the reference of the quantity of the *excretum* to the body-weight.
3. He limits his estimate to men between 20 and 40 years of age.
4. He rejects all determinations of quantity made on less than an average of three days; (and yet),
5. Admits experiments made by every variety of chemical manipulation; and
6. Made upon men of different countries, French, German, American, and English.

In the last two rules, we believe, the author has admitted sources of error and variations greater than those he has excluded by rule 4. The body of man in health is a well-ordered machine; and if it be made from day to day to do the same work, it will require the same amount of nutrition, and give rise to the same amount of waste; for this reason, we esteem of little value the painful experiments made by some observers, by taking an unusually large or small amount of food or exercise, and then determining the amount of excretions, such as urea, that have done their work in the system; for it is evident that the problem of the work done in the system by food or exercise is dynamical, and not statical, and that the body, under the same given conditions of food and work, acquires a stable condition of dynamical equilibrium, like the fly-wheel of a large steam-engine, which enables it to mask the effects of external forces applied to it, by virtue of the accumulated work which it has stored up. For this, and other reasons, we think that a single day's observation made upon a healthy man, who is living regularly, both as to food and work, is quite sufficient to determine his normal urinal constants. This, we believe, was the method followed by Mr. Haughton, in conducting the experiments published in this Journal, in August, 1859, and August, 1860; and, provided the condition of dynamical equilibrium be satisfactorily attained, we believe it to be quite sufficient for scientific purposes, and as satisfactory as eight or ten days' observation, with less attention to regularity of food and work would be.

We shall, therefore, compare the average results obtained

by Dr. Parkes, from men of different countries, and by different chemical methods, with those obtained by Mr. Haughton in his published papers; and the result of the comparison, we believe, will show that Mr. Haughton's method gives results, which do not differ materially (in most cases) from those given by Dr. Parkes; thus proving, as we think, that both methods are capable of giving the true means, while the advantage of simplicity, and fewer observations, belong to Mr. Haughton's method.

There is one very important omission in Dr. Parkes' results, viz., all mention of the particulars of food and work of the several subjects; this omission deteriorates from the value of his means, as it is known that, with variations of food and work, there are corresponding variations of urinary and other secretions.

The first excretion noticed is that of *water*, which is more readily measured than any other, and therefore there exists with respect to it less difference of opinion. Dr. Parkes gives (p. 5) the results of twenty-six observers, ranging from 35 ounces per day, to 81 ounces, the mean being **52.44** ounces daily.

Mr. Haughton found, by his method, a range from 34 ounces to 81 ounces, and a mean of **52.62** ounces daily,—the average of flesh-eaters being 47.3 ounces, and of vegetarians, 59 ounces daily.

This agreement of different methods is satisfactory, as it shows the true mean has been reached.

The second excretion, and the most important of all, is *urea*. Dr. Parkes' Table (including different chemical methods, and persons of different habits, and taking different foods) ranges from 286.1 grains to 688.4 grains per day; the mean being **512.4** grains, which is much nearer the higher than the lower limit, for reasons which might be easily assigned.

Mr. Haughton found a range from 315.00 grains to 677.25 grains, with a mean of **493.19** grains, which is almost exactly a mean between the extremes. This circumstance alone, we believe, entitles Mr. Haughton's method of inquiry to be considered of more value than the mixed method followed by the twenty-four observers quoted by Dr. Parkes.

Either method appears to give the correct mean; and, as the methods differ, they may be regarded as mutually confirming each other's results, when they do agree. If, therefore, we group together men of every weight, food, occupation, and country, we may set down **500** grains of urea per day, as the natural and healthy discharge in the urine.

It is a matter of the highest moment to refer the discharge of Urea to the Body-weight; this is done for nine cases by Dr. Parkes (at p. 23), where he obtains the result, that each pound avoirdupois excretes daily **3.36** grains of urea.

The average weight of the eleven persons experimented on by Mr. Haughton, was 148.1 lbs., which gives a result of **3.33** grains of urea daily per pound avoirdupois of body-weight. The agreement of this result with that of Dr. Parkes is very close.

The third excretion we shall consider includes pigment, extractives, creatine, and creatinine; these are estimated separately by Dr. Parkes, and, taken together, they give a result of **1.142** grains per pound per day (p. 24). Mr. Haughton has furnished us with the following table of the pigment, extractives, creatine, and creatinine, passed by his eleven cases, which were all brought beforehand into the condition of stable dynamical equilibrium of waste and supply:—

TABLE I.—*Extractives, &c., passed per day by Vegetarians.*

No.	Body-weight in pounds.	Extractives, &c. in grains.	Grains per pound.
1	173	260	1.50
2	132	236	1.79
3	146	135	0.92
4	146	99	0.68
5	146	118	0.81
Mean, . . .			<b>1.140</b>

TABLE II.—*Extractives, &c., passed by Flesh-eaters.*

No.	Body-weight in pounds.	Extractives, &c. in grains.	Grains per pound.
1	126	113	0.90
2	126	132	1.04
3	126	119	0.94
4	174	96	0.55
5	189	290	1.53
6	145	269	1.85
Mean, . . .			<b>1.135</b>

The agreement of these means with that published by Dr. Parkes leaves nothing to be desired.

With regard to *Uric* and *Hippuric* acids, there is a striking difference between the results given by Dr. Parkes and those obtained in this city by Mr. Haughton.

Dr. Parkes gives ~~8.589~~ grains per day of uric acid as the quantity obtained by fourteen observers, and mentions quantities of hippuric acid ranging from this amount to 34.5 grains per day! as found by Lehmann and Weismann.

These results are certainly at variance with our experience of the urine of Irishmen, who pass, on an average, only 3.15 grains of uric acid, and no hippuric acid at all, except in rare cases. Can it be possible that the beer-drinking of the Germans tends to produce these abnormal compounds, particularly hippuric acid? If this be so, Charles the Fifth was not far astray, when he said he would speak German to his horse; for there appears to be a closer connexion between the frames of Germans and horses than has been hitherto suspected.

According to our judgment, both these acids are chance compounds, occurring in the urine of man as accidental portions of nitrogen that have escaped their natural conversion into urea, and as such must be regarded as essentially unhealthy products.

The mean amount of *sulphuric acid*, found by nineteen observers, is stated by Dr. Parkes to be 31.11 grains per day. Mr. Haughton's observations on this subject are limited to four vegetarians, from whose daily urine he found the following results:

TABLE III.—*Sulphuric acid passed per day by Vegetarians.*

No.	Weight in pounds.	Sulphuric acid in grains.	Grains per pound.
1	132	40.65	0.308
2	146	18.88	0.129
3	146	21.00	0.144
4	146	23.50	0.161
Mean .		26.01	0.185

The only inference we feel disposed to draw from the comparison of this table with that of Dr. Parkes is, that either the

Dublin bread contains less alum than German bread, or that we eat less bread than they do.

A similar observation may be made with respect to the quantity of *chlorine* passed per day, which depends rather on the habit of eating more or less salt with the daily meals—a habit which differs in every individual.

Dr. Parkes states that, as the result of sixteen observers, **126.76** grains of chlorine are passed per day. Mr. Haughton's observations on seven cases are irreconcilable with this statement, if we suppose the discharge of chlorine to have any physiological significance. They are as follows:—

TABLE IV.—*Chlorine passed per day by Healthy Men on a Mixed Diet.*

No.	Chlorine in grains.
1	26
2	49
3	13
4	116
5	40
6	36
7	79
Mean . .	<b>51.3</b>

With respect to *phosphoric acid*, which is of undoubted physiological importance in the urine, a serious difference exists between Dr. Parkes' table, p. 15, and the results published by Mr. Haughton in this Journal.

The total discharge of phosphoric acid per day, according to Dr. Parkes, amounts to **48.8** grains, obtained by twenty-five observers, the majority of whom, however, obtained their results by the use of Breed's method (phosphate of iron). Our own experience of this method is not such as to induce us to place much reliance upon it, as the precipitate appears to be of variable composition, according to varying conditions of precipitation; and therefore the phosphoric acid calculated by this method must be regarded as doubtful.

Mr. Haughton has informed us that all his results with

regard to phosphoric acid were obtained by actual weighing of the pyrophosphate of magnesia, and that he does not believe there is any reason to suspect any defect of phosphoric acid in them. These results are—for flesh-eaters, 37·07 grains per day, and for vegetarians, 26·7 grains; the mean of all being **32·36** grains per day. This result falls very short of that given by Dr. Parkes; and the difference cannot be explained, like that of sulphuric acid and chlorine, by accidental differences of diet; for the phosphoric acid unquestionably performs a more important office in the working of the animal frame than either of the others, and leaves the body in a condition different from that in which it enters it.

Notwithstanding the discrepancy between Dr. Parkes' and Mr. Haughton's results as to the total amount of phosphoric acid, there is an excellent agreement between the table (p. 17), which gives the relative proportions of phosphoric acid in combination with alkalis, and in combination with earths, and the corresponding relation deducible from Mr. Haughton's experiments:—

Ratio of phosphoric acid in combination with alkalies, to phosphoric acid in combination with earths, according to the results of seven observations published by Dr. Parkes, . . .	4·29
Ratio, &c., &c., according to Mr. Haughton's experiments, . . . . .	4·15

This fact serves to show that the difference in the total amount of phosphoric is real, and possibly attributable to the greater amount of phosphoric acid taken in food by Dr. Parkes' observers, the greater number of whom are Germans.

The last point on which we have to compare Dr. Parkes' tables with the facts published in this Journal by Mr. Haughton, is as to the total amount of *solids* passed per day.

Dr. Parkes gives **945** grains as the mean of nine observers; while Mr. Haughton's mean is **957·81** grains per day. This agreement is what we should have expected from the previous agreement of their estimates of water and urea.

In the following table we sum up the results of Dr. Parkes (p. 24) and Mr. Haughton:—

TABLE V.—Average discharge of Normal Constituents of healthy Urine per day, and per pound of body-weight.

Constituent.	Ounces, or Grains, per day.		Drachms, or Grains, per pound.	
	Parkes.	Haughton.	Parkes.	Haughton.
1. Water . . . .	52·44 oz.	52·62 oz.	2·90 drs.	2·84 drs.
2. Urea . . . .	512·40 grs.	493·19 grs.	3·36 grs.	3·33 grs.
3. Extractives, &c.	165·59 „	169·70 „	1·142 „	1·138 „
4. Uric acid. . . .	8·591 „	3·15 „	0·059 „	0·021 „
5. Sulphuric acid .	31·11 „	26·01 „	0·214 „	0·182 „
6. Chlorine . . . .	126·76 „	51·28 „	0·875 „	0·346 „
7. Phosphoric acid	48·80 „	32·36 „	0·336 „	0·218 „
8. Unaccounted for	51·75 „	182·12 „		
Total solids . .	<b>945·00</b>	<b>957·81</b>		

On examining this table, a difficulty suggests itself with reference to the solids unaccounted for. These must be the bases in combination with the various acids of the urine, and are soda, potash, ammonia, lime, and magnesia. Dr. Parkes' table leaves 51·75 grains of these bases, while Mr. Haughton has to explain 182·12 grains. Neither of these quantities, we believe, can be explained, as we shall now proceed to show. The principal base is soda, the atomic weight of which is not very different from the mean of those of all the bases. We shall therefore assume soda as the type base in the following calculation.

The acids given by Dr. Parkes require the following quantities of base to saturate them:—

<i>Acids.</i>	<i>Bases.</i>
31 grains $\text{SO}_3$ require	24 grains $\text{NaO}$
127 „ $\text{Cl}$ „	83 „ $\text{Na}$
49 „ $\text{PO}_5$ „	42 „ $\text{NaO}$
<b>207 grains</b>	<b>149 grains</b>

It appears, therefore, that the acids would require 149 grains of base; whereas there are only 52 grains of solids unaccounted for, and available for this purpose. This seems to us as if Dr. Parkes had estimated too highly the amount of acids in the urine.

On the other hand, Mr. Haughton's acids are not sufficient to account for the balance left unexplained in his table:—

<i>Acids.</i>		<i>Bases.</i>	
26 grains	SO <sub>2</sub>	require	20 grains NaO
51    "	Cl	"	34    " Na
33    "	PO <sub>5</sub>	"	38    " NaO
<hr/> 110 grains		<hr/> 92 grains	

This calculation gives only 92 grains to explain the presence of 182 grains of solids not accounted for.

From the preceding reasoning, it would appear that Dr. Parkes has estimated the sulphuric acid, chlorine, and phosphoric acid discharged daily at too high a figure; and, on the contrary, that Mr. Haughton has estimated them at too low a rate. Unfortunately, we cannot tell in which of the constituents the error of either writer consists. If we were to hazard an opinion, it would be that Mr. Haughton has not given enough chlorine, and that Dr. Parkes has overestimated the chlorine and phosphoric acid.

The following considerations, based on Mr. Haughton's determination of the fixed salts in the daily urine of ten of his subjects, serve to throw some light on this interesting question:

TABLE VI.—*Alkaline and Earthy Fixed Salts passed per day.*

No.	Alkaline salts, in grains.	Earthy salts, in grains.	Total in grains.	Body-weight, in lbs.
1	214.54	23.46	238.00	126
2	221.94	22.68	244.62	126
3	222.04	11.96	234.10	126
4	364.00	20.30	384.30	173
5	145.00	15.00	160.00	174
6			405.00	189
7			297.25	145
8			421.20	132
9			261.00	146
10			249.20	146
Mean	233.50	18.68	252.18	

From this table it appears that the salts of soda, potash, lime, and magnesia, passed by the kidneys per day, amount to



~~289-46~~ grains. Adding together the acids and bases of Dr. Parkes' and Mr. Haughton's results, we find—

Fixed salts (according to Dr. Parkes)	. . .	356 grains.
„ (according to Mr. Haughton)	. . .	202 „

This consideration shows an excess of 67 grains in Dr. Parkes' result, and a deficiency of 87 grains in Mr. Haughton's figures.

We believe that further experiments will be necessary to decide where the error lies; but that there is an error somewhere in the estimates of both writers, is, we think, certain.

Mr. Haughton's amounts of phosphoric and sulphuric acids were invariably determined by weighing, whereas his amount of chlorine was found by the volumetric process with nitrate of mercury. This process he never found to be satisfactory, as no two observers could agree when the precipitate ceased to be redissolved. For this reason, he thinks that the deficiency of acids rests with the chlorine, which ought to be about 100 grs. instead of 50 per day, in order to account for the balance of the solids.

From the preceding comparison of Mr. Haughton's determination of the constants of healthy urine, published in this Journal, with the results published by Dr. Parkes, it is evident that the most satisfactory agreement exists between these writers as to the mean amount of water, urea, extractives, and solids passed per day, by average men in health, with every variety of food and work; from this point, however, the two writers diverge,—Mr. Haughton laying more stress on the *occupation*, bodily and mental, of the subject considered, and Dr. Parkes attributing great importance to the *food*. If, however, we reflect that the occupation must regulate the food, the difference between them is less than would appear at first sight.

Dr. Parkes, in section xxvii. of Chapter I., renders ample justice to Mr. Haughton's speculations as to urea considered as the measure of work done. As these speculations are familiar to our readers, we shall pass over Dr. Parkes' account of them, observing only, that he does not agree with Mr. Haughton, in the opinion that mental work causes an increased elimination of urea.

Let us turn to the practical application of the facts laid down in the first part of Dr. Parkes' book. In pages 231-2, he lays down five rules for the determination of the physiological discharge of urea by a patient, under given circumstances—the amount of this discharge must be always calculated, before we

can determine whether he is passing too much or too little of urea—or of any other constituent.

These rules are—

1. Find weight in pounds.
2. Multiply the weight by given coefficients, to find mean healthy discharge of constituents.
3. Correct result for youth, age, and sex.
4. Correct for movement ( $\frac{1}{10}$ th to  $\frac{1}{20}$ th).
5. Correct for diet ( $\frac{1}{10}$ th to  $\frac{1}{100}$ th), ( $\frac{1}{3}$ rd, if fasting).

Dr. Parkes gives the following case in illustration (in quoting the case, we have corrected a typographical error):—

“I subjoin a case of mine, to show the working of the plan. A healthy man, aged thirty, on moderate hospital diet, weighed 114 lbs. Required the physiological amount of urea in twenty-four hours—

$$3.53 \times 114 = 402.42 \text{ grains.} \quad (1)$$

“Correct for moderate diet; deduct  $\frac{1}{10}$ th,

$$402.42 - 50.30 = 352.12 \text{ grains.} \quad (2)$$

“Correct for bodily inactivity; deduct  $\frac{1}{20}$ th,

$$352.12 - 17.60 = 334.52 \text{ grains per day.} \quad (3)$$

“The physiological amount determined by actual experiment (mean of six days) in this man was 346.7 grains, being only 12 grains above the calculated amount.”

This calculation is, no doubt, satisfactory; but it may be made from a totally different point of view, as follows:—

From Mr. Haughton's Table I., it can easily be inferred that the *opus mechanicum*, in the average, may be considered to be represented by **66.74** grains of urea per day. Dr. Parkes deducts for total rest  $\frac{1}{10}$ , and for quietude the half of this, or  $\frac{1}{20}$ th of the whole urea—this is equivalent, on Mr. Haughton's theory to deducting 33.37 grains; and as the co-efficient for urea is 3.33, we find—

Healthy average urea per day =  $3.33 \times 114 \text{ lbs.} = 380 \text{ grains.}$

Deduct for quietude, . . . . . 33.37 „

---

**346.63** gra. per day.

This result is absolutely identical with that found by actual trial.

We believe, however, that the following rules, based on Mr. Haughton's theory, will be found the simplest and most accurate in practice, for determining the physiological amount of urea in a given case:—

1. Take the body-weight in pounds.
2. Multiply by 2, to find the *opus vitale* in grains of urea.
3. Add 33 grains for quietude, when there is no mental work.
4. If there be mental work, or anxiety, imagine an unknown quantity added.

To illustrate this view, we give the following cases, both taken from hospital patients, bordering on the uræmic condition:—

CASE I.—G. B., Adelaide Hospital, aged 48 *circiter*, passed 31 ounces of urine in twenty-four hours, free from albumen and sugar, sp. gr. 1015·5; had had albuminuria some months before; symptoms of uræmia impending. The total quantity of urea in the 31 ounces of urine was 305 grains; and body-weight, 137 lbs.

Applying the preceding rules, we find—

$$\text{Opus vitale} = 137 \times 2 = 274$$

$$\text{Add for quietude} \quad . \quad . \quad 33$$

---

**307 grains of urea.**

CASE II.—J. M'C., Sir Patrick Dun's Hospital, aged 50, *circiter*; body-weight, 134 lbs.; symptoms like last; never had albuminuria; trace of sugar in urine; passed  $7\frac{1}{2}$  pints of sp. gr. 1002·9; containing 261·4 grains of urea, lying in bed.

$$\text{Opus vitale} = 134 \times 2 = 268$$

$$\text{Add for total rest} \quad . \quad . \quad 000$$

---

**268 grains of urea per day.**

These two cases must be considered as bordering on the minimum discharge of urea, consistent with existence; they were both on a diet better than the average of hospital diet; and we do not see how they could be dealt with by Dr. Parkes' rule; neither case was in the uræmic condition, but both decidedly bordered upon it.

Dr. Parkes' rules would give the following results in these two cases:—

CASE I.—Healthy urea . . .  $137 \times 3\cdot53 = 483\cdot6$  grs. of urea.

Deduct for diet ( $\frac{1}{8}$ th) . . . 60·4 grains.

Deduct for quietude ( $\frac{1}{10}$ th) . . . 24·1 „

Deduct for age ( $\frac{1}{10}$ th) . . . 48·3 „

---

132·8

---

**350·8 gra. per day.**

This is 46 grains above the truth.

CASE II.—Healthy urea . .  $134 \times 3.53 = 473.0$  grains.

Deduct for diet ( $\frac{1}{8}$ th) . . . . 59.1 grains.

„ total rest ( $\frac{1}{10}$ th) . . . . 47.3 „

„ age ( $\frac{1}{10}$ th) . . . . 47.3 „

— 153.7

319.3 grs. per day.

This healthy physiological result is 58 grains above that actually found.

We believe the excess, resulting from Dr. Parkes' calculation applied to both these cases, is real, and represents the average *mental work* per day, which is already taken into account in his co-efficient 3.53; but which was altogether absent in the patients under consideration.

From the length at which we have examined Dr. Parkes' results as to the normal constituents of the urine of health, we have but little space left for the consideration of the constituents of the urine of disease. We would especially direct the attention of the student to his sections (p. 244) on typhoid fever, (p. 270) on pneumonia (acute lobar), and pleurisy (acute).

The discharge of urea in pneumonia is greatly above the average, having been found, as a mean of twelve cases, to be **354** grains per day; while in pleurisy it only amounted to **348** grains, in a carefully examined case of a young man, the solids in the same case being only **482** grains.

The discussion of the phenomena of diabetes mellitus (p. 337) is the best we have seen.

The principal deductions to be made from this discussion are as follows:—

1. *Urea*.—There is an excretion of **311** grains daily (mean of twelve cases).

2. *Uric and Hippuric Acids*.—Not much altered.

3. *Sulphuric Acid*.—Greatly increased in amount.

4. *Phosphoric Acid*.—If altered at all, lessened in amount.

5. *Chlorine*.—Excretion of **247** grains daily (mean of seven cases).

These results we consider to be on the whole correct, with the exception of the asserted diminution of phosphoric acid. We have found this constituent increased largely, and in proportion to the other constituents of healthy urine. We shall take some other opportunity of stating our views on this subject, as well as on the proportion of sugar to urea, which is well discussed in pages 351–2.

Dr. Parkes admits three varieties of diabetes insipidus.

1. *Polydipsia*.
2. *Diuresis*, with deficient action of the skin and lungs.
3. *Diuresis*, occurring during the removal of dropsical affections.

On the first of these, we shall make a few observations. With regard to the assertion, that the urine exceeds the water of the liquid and solid food, we believe it rests on imperfect observation and calculation of the fluid in the solid food. According to Dr. Parkes, there are three varieties of polydipsia. They are the following:—

(a). *Polydipsia* with hydruria, and anazoturia, or deficiency of urea, and sometimes of phosphoric acid.

(β). *Polydipsia* (of Becquerel), with simple hydruria.

(γ). *Polydipsia*, with hydruria, azoturia, or increase of urea, and generally of chlorine—sometimes of phosphoric and sulphuric acids.

Dr. Parkes considers the variety (β), in which the only constituent changed is the water, to be of very doubtful occurrence. We have met with one such case, well marked; but consider it to be very rare.

In the third variety (γ) of polydipsia, the most marked increase in the constituents (in addition to the water) is in the chlorine; this increase is certainly real in the cases recorded by Dr. Parkes, as its amount greatly exceeds his average of health (126·8 grains) which we believe to be, itself, above the true average.

Our own limited observations on this form of diabetes confirm the accuracy of Dr. Parkes' conclusions.

We close our remarks, as we began them, by an expression of our opinion, that this book of Dr. Parkes is well timed, well written, and that it will rapidly take a high place among the scientific works which treat of the important subject with which it deals.

*A Clinical Treatise on Diseases of the Liver.* By DR. FRIED. THEOD. FRERICHs, of the University of Berlin. In 2 vols. Vol. I. translated by Charles Murchison, M.D., for the New Sydenham Society. London. 1860. 8vo, pp. 402.

THIS excellent work, so creditable to the distinguished author, whose reputation is scarcely less in these countries than in Germany, is one of the best productions of the New Sydenham Society. We look forward to its completion with the utmost interest, and are glad to observe that the subscribers to that most

useful society may confidently expect the early issue of the second volume. A short, but very satisfactory, "historical introduction" is a fitting preliminary to the work. The most curious subject for reflection, in connexion with this branch of the inquiry, is the diversity of views concerning the importance of the liver in the animal economy at various periods of medical history. The notions of Galen, which seemed so extravagant to the anatomists of the seventeenth century, have now again been upheld by the most recent and authoritative researches:—

"Galen looked upon it as the focus of animal heat, and as the organ intended for the formation of blood, and for the origin of the veins. According to him, the metamorphosis of chyle into blood commenced in the portal veins, but was completed in the liver, which organ, during the process, separated, as waste matter, from the blood, the yellow and the black bile,—the former passing to the gall-bladder; the latter, to the spleen."

Much of this statement seems to be now irrefragably based on fact; but the very outline of the dogmatic pretensions of the physiologists here placed before the reader, should teach a lesson of modesty, and induce us to pause before yielding an unlimited assent to the views now dominant.

The second chapter, on "the relative size and weight of the liver in health and in disease," consists mainly of a series of tables, in which are arranged the results of the author's observations. We may in vain search elsewhere for so extensive and accurate a series of researches on these important subjects. They form a necessary groundwork for the elaborate clinical inquiry which immediately follows, and must be studied with care by those who would derive all the advantage the book is calculated to impart. The modifying influences of "sex," "age," "ingestion of food," and "degree of congestion of the liver," are successively alluded to. Here, too, the strange contradictions among the authorities strike the eye, even when involving questions of weighing and measuring. Bartholin estimated the relative weight of the liver in proportion to that of the body, as 1 to 36. Haller, as 1 to 25. The average weight of the gland was considered by Haller to be 3.7 lbs. Cruveilhier's estimate was 3 lbs. "According to my experience," says the author, "the relative weight of the liver in healthy individuals may vary from one-seventeenth to one-fiftieth of that of the body; and in adults it fluctuates between one-twenty-fourth and one-fortieth; the absolute weight at

this period of life reaches from 1·8 to 4·6., English lbs. avoirdupois."

These numbers certainly exhibit a wide range of variation within healthy limits; and the known care of the author gives a sufficient guarantee for the accuracy of the statements; but we believe that few clinical observers, in their efforts to determine the hypertrophy or atrophy of the liver, pay sufficient attention to these fundamental facts.

The ingestion of food exercises a powerful influence over the volume of the liver. Dr. Frerichs found that in a healthy individual, aged 27, who, owing to a fall from a scaffold, died with a full stomach, the relative weight of the liver was found to be as 1 to 26·5; in another person, aged 36, who perished under similar circumstances, it was as 1 to 37; on the other hand, in a man aged 25, who died of trismus, after three days' complete abstinence, it was 1 to 40; and in a woman, 33 years of age, who died from burning the pharynx with sulphuric acid, after seven days' fasting, it was 1 to 50. These figures offer indisputable proof of the vast influence of the digestive processes on the liver,—a doctrine which indeed is universally entertained, even among the vulgar. But, as is judiciously noted by the author, we also infer from them that a strict diet must play an important part in the management of chronic congestions of the liver. In one sense, this, too, is a pretty generally diffused dogma, though it may be doubted if the considerations connected therewith meet with the practical assent they deserve.

The third chapter contains a sketch of the relative sizes and forms of the liver in its morbid conditions, and of their diagnostic value; elaborate tables illustrate the subject; and the varied information obtained by palpation, percussion, and inspection, is fully set forth. Both here and in other parts of the book, great attention is bestowed on the tight-lace liver: much more than is usually allowed to this artificial anomaly in English works on the liver. In reference to the *mobility* of tumours connected with this organ, it is observed, that "in the case of the lobes separated by tight-lacing, it is not unfrequently so great, that we can turn the tumour, and lay it upon the upper surface of the organ. In exploring the hepatic region of females, this observation merits to be borne in mind, so long as women try to de-naturalize the feautiful frame with which nature has gifted them.

Cruveilhier's remark, that a liver whose position has been changed by tight-lacing may be diagnosed from that which results from inflammatory enlargement, by the thinness of the

border in the former case, is stated by Frerichs to be an error, inasmuch as the margin of the tight-lace liver is always rounded and nodulated.

The section on the application of the results of observation to diagnosis is judicious, but presents no novelty. It is insisted upon that structural diseases of the hepatic parenchyma give rise to abnormal positions of the organ, only when they are accompanied by softening. This is the case in fatty degeneration, and in acute atrophy. The effects of these diseases vary according to the degree of softening. When this is moderate, as may happen in fatty liver, the axis falls downwards, and the gland protrudes farther below the margin of the ribs. Hence a fatty liver usually appears, upon percussion, to be larger than it really is. When the softening is considerable, as in acute atrophy (in which the gland is flexible, like a piece of cloth), the liver folds upon itself, and collapses towards the vertebral column, and the space corresponding to it in front is filled up by intestines containing gas. The hepatic dullness then disappears entirely in front, whilst posteriorly it may still be detected (p. 50). This last is a most weighty point as regards the diagnosis of acute atrophy, the most formidable and the most obscure of hepatic disorders. Scarcely any other deviation from the normal state is sufficiently characteristic to enable us in ordinary cases even to suspect the existence of that disease. The diagnosis of increased volume of the liver from empyema is, in some cases, surrounded with many difficulties; but the author draws attention to the valuable aid derived from a careful determination of the *mobility* of the liver on a deep inspiration. Here, of course, we are mainly guided by its lower margin. When the mobility remains unchanged during a full inspiration, we infer that the diaphragm, and, with it, the liver, are not depressed by pleuritic effusion, but that an increase in size of the organ has altered the situation of its upper and lower margin. The reverse side is not always true, as in some cases the diaphragm loses its contractibility through fatty or fibrous degeneration.

The fourth chapter consists of an extended discussion on jaundice, wherein the author's originality and masterly clinical knowledge are admirably displayed. We are unable to do justice either to the writer or to the subject, without giving an extended *resumé* of this part of the treatise.

It is justly stated *in limine* that, although copious materials have been accumulated, in the course of time, bearing on this subject, yet the most important fundamental questions have never received a satisfactory explanation. Passing over



minor detailed views, we may easily resolve past opinions into the two following theories:—

1. One of these theories is, that the seat of these diseases is to be looked for in a disturbance of the functions of the liver; that they arise from abnormalities in the secretion, or in the excretion of bile, and that thus they exhibit symptoms indicative of derangement of the liver.

2. The second theory is that, under morbid conditions of the system, substances are formed in the blood without the co-operation of the liver, which, in colour and other properties, resemble the ingredients of bile, if they are not identical with them, but which only assume a pathological importance from their quantity being in excess (p. 79).

The first of these theories is the time-honoured one of Hippocrates and Galen; and, in spite of numerous revolutions, has held its ground to near the present time. That obstruction to the excretion of bile is a cause of jaundice, may be considered a settled doctrine; but, in the absence in certain cases of material proof of such obstruction, how can the jaundice be accounted for? The explanation of such forms which has obtained general assent in this country is that which has received the sanction of Drs. Watson and Budd, namely, that the bile does not originate in the liver, *but is previously generated in the blood*, and that thus anything which interferes with its *elimination* may give rise to jaundice. It is remarkable that this last view is still maintained, in the face of the chemical analysis of portal blood, and of the experiments upon the extirpation of the liver.

The second theory has also had supporters from an early period of medical history. Senac held that the red portion of the blood was the peculiar material of the bile, and that it assumed a yellow colour when it became putrid, or otherwise decomposed. Lately, since the doctrine that the hematine of the blood forms the basis of all pigments has become more generally accepted, many observers have referred the jaundiced tint of the skin, present in pyemia, in putrid infection, and other allied conditions, without any morbid condition of the liver, to a direct metamorphosis of the hematine into a yellow substance, similar to, if not identical with, bile-pigment. This view has obtained fresh support from the experiments of Virchow, which show that, under certain circumstances, a yellow substance is formed from the hematine, which, in its relation to solvents and re-agents, bears a close resemblance to cholepyrrhin. The discovery of Zenker and Funke, that bilifulvine can easily be transformed into hematoidine (a derivative of he-

matine), also shows an intimate relation between the bile-pigment and the red matter of the blood. Further, we are told in the preface that, "since the publication of the German edition of the first volume, certain experiments have been performed in Frerich's laboratory by his assistant, Dr. Valentin, which tend to show that one of the colouring-matters of bile consists of hematine, the substance which is known to be derived from blood-pigment. Valentin has succeeded in detecting crystals of hematine in gall-stones, in the bile of men and animals, and in the tissues and secretions of jaundiced patients. The addition of chloroform is found to dissolve the hematine with a yellow colour; and, from this solution, red and brownish-red, lancet-shaped, and rhomboidal-prismatic crystals separate, which correspond in every respect with those of hematine" (p. xviii.). These two theories of bilious affections have never received a universal application, and were employed to explain the origin of those forms of jaundice only, in which no proof existed of any obstruction to the escape of bile. Their main value consists in the necessity for some hypothesis for explaining observations where no anatomical obstruction can be detected. "In such cases can we ascribe the jaundice to an accumulation of bile in the blood, owing to something which interferes with its secretion; or are we to adopt the theory of a direct crumbling down of the blood-corpuscles, or red matter of the blood, into bile-pigment?" (p. 82). There are numerous well-established facts which seem to negative the opinion of Budd, that an imperfection in the secreting functions of the liver may be the cause of jaundice. All the efforts made to detect the essential elements of the bile in the blood generally, and in that of the portal vein in particular, have been nugatory. Frerichs relates a case (No. xxix.) where the secretion of bile was completely arrested in consequence of fatty degeneration of the liver, in which the gall-bladder was found empty, and the biliary ducts coated with a greyish mucus; and, notwithstanding, the skin was of a chalky paleness, and the urine not tinged with bile-pigment. But the most decided proofs of the formation of bile in the liver are afforded by the experiments of Kunde and Moleschott, which show that not a trace of the elements of bile could be detected in the blood, lymph, urine, or muscular tissue of frogs, several weeks after they had been deprived of their livers.

As regards the second hypothesis, the spontaneous conversion of the red matter of the blood into bile-pigment, no objection can be made on the score of possibility, considering the known intimate relations of hematine and cholepyrrhin. But

no one has yet succeeded in manufacturing bile-pigment from the red colouring matter of the blood. It remains to be proved that this metamorphosis really takes place in the living body. How, then, are those cases of jaundice to be explained which occur without any remarkable obstruction to the excretion of bile—cases which include, in addition to the jaundice of pyemia and of allied conditions, the jaundice of chloroform and other forms of intoxication, that of pneumonia, intermittent fevers, bilious fevers of marshy districts, inflammation of the portal vein? It is necessary here to go back, and search after the causes which can give rise to accumulations of bile in the blood. Excluding, now, polycholia, and assuming the formation of bile to be a constant quantity, these causes may be said to be twofold:—

1. Through increased absorption of bile from the liver into the blood.

2. Through some alteration in the metamorphosis of substances contained in the blood\*.

The mechanism of biliary obstructions is more complicated and obscure than is generally imagined. We must conclude that an increased absorption of bile into the blood is dependent upon a difference in tension of the contents of the hepatic cells and blood-vessels. Such a condition may arise either from obstruction of the bile-ducts, by which the pressure on the side of the cell-contents is increased, or from obstruction to the flow of blood in the portal system, and consequent diminution of the pressure on the side of this fluid. All mechanical obstructions, interfering with the flow of bile into the larger or smaller biliary ducts, produce jaundice in the first of these two ways. The fundamental principle is *increased pressure* on the side of the bile. Its outflow seems principally brought about by the *vis a tergo* of the secretion constantly pressing onwards. Muscular contraction, causing spasm of the ducts, has been long considered a cause of jaundice; but this, apart from the existence of a local cause, as a concretion, must be negatived. Paralysis of the ducts has been invoked to the same end. All *a priori* argument is against this supposition, and the direct experiments of Frerichs and Bernard contradict it. In reference to the circumstances influencing the flow of bile, we must not leave out the compression exercised upon the abdominal organs by the respiratory movements. The

\* The reader must take care to make an important correction in the text, and refer to the errata. Since the publication of the first German edition, new researches have led the author to modify previous statements.

want of this influence may be insufficient to give rise to jaundice, but it may co-operate in the production of the result.

The second mode by which bile is made to enter the blood, by the obstruction to the circulation, and the consequent diminution of the pressure on the side of the capillaries of the portal vein, is estimated with difficulty. There can be no doubt, however, that the tension of the capillary vascular system in the liver is diminished, and the entrance of the biliary contents of the hepatic cells into the blood facilitated, by obstruction of the main trunk, or of the larger branches of the portal vein. Here we find an explanation of the jaundice of new-born children, a diminution of the tension of the hepatic vessels occurring, directly after birth, when the portal vein ceases to receive blood from the umbilical veins. The great hemorrhages of yellow fever must also produce variations in the pressure exerted on the side of the hepatic vessels. The importance of the distribution of the blood in the liver must not, therefore, be lost sight of in the production of jaundice.

We arrive now at the *second* cause of the accumulation of bile in the blood—that arising through some alteration in the metamorphosis of substances contained in the blood. The original researches of the author are prefaced by the following statement, which we extract from the page of errata:—

“The bile-pigment is so intimately related, on the one hand, to the red matter of the blood, and, on the other, to the colourless biliary acids, as to justify us in referring its origin to one or the other of these sources.

“The intimate relation subsisting between the bile-pigment and the colouring-matter of the blood is indicated by facts which have been already mentioned, but more particularly by observations which have been recently made in my laboratory by Dr. Valentin, according to whom, a portion of the colouring-matter of the bile dissolves in chloroform; and from this solution a crystalline substance may be obtained, presenting all the characteristics of hematoidine. From this it appears possible, nay, probable, that, as in extravasations, hematoidine may be developed from blood-pigment; so, in like manner, in the vascular system, and in the liver, the colouring-matter of bile may originate from the same source. Hitherto, however, no one has succeeded in obtaining bile-pigment directly from the red matter of the blood. The second view rests upon the following facts:—The pure colourless acids of the bile may be transformed into *bile-pigment*, with all the properties characterising this substance. Such a transformation takes place, not only under the influence of re-agents, but it also follows the absorption of the acid substance (into the blood of living animals), and is in a measure dependent upon this. By the action of concentrated sulphuric-acid

upon colourless bile, there are formed colour-producing substances (chromogene), which, upon exposure to the atmosphere, and still more rapidly on the addition of nitric acid, exhibit alternations of tints corresponding, in every respect, with bile-pigment. The same pigments and colour-producing substances (chromogene), which in their properties precisely resemble cholepyrrhin, are produced by the injection of large quantities of colourless bile into the vascular system of living animals. In this case the acids of the bile are transformed in the blood into pigment, under the influence of respiration."

These remarks are supported by numerous experiments, fully detailed in the appendix. Certain facts suggest the supposition, that, under various pathological conditions, the metamorphosis of bile in the blood is incomplete, and that thus a sufficient quantity of bile-pigment remains in the blood to give rise to all the symptoms of jaundice.

This theory of jaundice has been impugned by Dr. Kühne, since the appearance of Frerichs' work; an abstract of his paper may be found in Dr. Beale's Archives of Medicine, vol. i. This physician denies that the biliary acids are decomposed in the blood; and asserts that, in jaundice from closure of the ductus communis choledochus, the urine *always* contains biliary acid, as well as bile-pigment (see preface). Dr. Kühne's explanation of Frerichs' experiment, which showed a great quantity of colouring-matter in the urine after the injection of biliary acids into the blood, is, that the colouring-matter which is developed under these circumstances is due to the property possessed by the biliary acids of dissolving the blood-corpuscles, and of thus setting free a quantity of hematine, which is converted into bile-pigment.

The results attained by Frerichs have been confirmed by many subsequent observers, especially by Professor Staedeler, of Zurich, and Dr. Neukomm. This latter experimenter has availed himself of a test more delicate than either Pettenkofer's or Hoppe's (which was used by Kühne). We now quote from the lucid preface by the translator:—

"These several processes were applied to the examination of two specimens of jaundiced urine from the human subject, and to that of the urine of seven dogs, into whose blood solutions of the biliary acids had previously been injected. In both the specimens of jaundiced urine, Pettenkofer's test gave a negative result; but the author's process indicated the presence of minute traces of the biliary acids. It is shown, however, that the quantity of these acids was so small (less than one-thirteenth of a grain troy in forty-two fluid ounces) as to bear a very small proportion to the amount

which must have entered the blood. Of the seven dogs, into whose blood *large quantities* of the colourless solutions of the biliary acids were injected, in all the urine became coloured with bile-pigment within a certain number of hours after the operation; in none could any bitter taste be distinguished in the urine; in none did the urine exhibit the slightest reaction with Pettenkofer's test; and in two cases only did the author's test indicate the presence of traces of the biliary acids in this secretion."

Now, let it be observed that Kühne's experiments fail in proving that the colouring-matter of the urine originates from the colouring-matter of the blood, and not from the transformation of the biliary acids; and they likewise fail in accounting for the disappearance of the biliary acids injected into the blood, in any other manner than that suggested by Frerichs' (Translator). A high probability, therefore, exists in favour of the author's doctrines of the genesis of icterus, which he thus sums up:—

"We have become acquainted with three causes of icterus:—

"1. Obstruction to the escape of bile.

"2. Diminished circulation of blood in the liver, and consequent abnormal diffusion.

"Both of these conditions give rise to an increased imbibition of bile into the blood; and, in both cases, the liver is more or less deeply implicated.

"3. Obstructed metamorphosis, or a diminished consumption of bile in the blood.

"This cause is independent of the liver; and, so far as we as yet understand the matter, is chiefly influenced by the composition of the blood, and by everything which essentially limits or modifies the processes of metamorphosis within the vascular system."

The next 100 pages of the treatise are occupied with an elaborate clinical inquiry into the various forms of jaundice, the subject being illustrated by numerous cases. We had marked many parts for quotation, but must now hasten on to the remaining chapters.

The fifth chapter deals with suppression of the functions of the liver, acholia, and its consequences. Here, too, jaundice is one of the symptoms of the disease; but this form of disorder always comes on sporadically, depends upon a suspension of the secretion of bile, and, as a general rule, is produced by acute atrophy of the liver, and in some instances by other structural diseases, such as cirrhosis, fatty degeneration, &c. It is much more dangerous than the former varieties of jaundice, and almost always terminates fatally. In infectious diseases, attended with jaundice, enlargement, and albuminous infiltra-

tion of the liver and kidneys, are not unfrequently present. This constitutes the commencement, from which proceeds destruction of the hepatic cells. In this way, typhus and allied processes may become the remote causes of acholia.

The acute or yellow atrophy of the liver is a disease still but imperfectly known; and what accurate information we have concerning it, dates no farther back than the time of Morgagni. It must be admitted, too, as observed by the author, that the present materials at hand for enabling us to comprehend the precise characters of the malady, are of a very heterogeneous nature. The symptoms are sometimes preceded by a preliminary stage, but at other times manifest themselves directly. In severe cases, the scene terminates at the end of twelve or twenty-four hours; in other cases, after two or five days; it is scarcely ever prolonged for a week. When precursory symptoms exist, they present nothing characteristic. Sooner or later, a slight jaundiced tint of the skin supervenes on the other pyrexial and gastro-enteric phenomena. This jaundice may exist in the simple form from eight to fourteen days, or even longer, before the local changes in the liver and spleen, the hemorrhages, and the serious derangements of the nervous functions, which characterise the affection, become apparent. The fully-developed disorder is attended with cephalalgia, delirium, generally of a noisy character, convulsions, and coma. The pupils become larger, react slowly with light, and the respiration becomes sighing, intermittent, and stertorous. The pulse, *at first, slow*, at the outbreak of the nervous symptoms increases in frequency, and gradually rises to 110 or 120. It also presents remarkable variations in its frequency and volume. Towards the close, the pulse always increases in frequency, and becomes smaller. In most cases, the abdomen is tender on pressure; even during the coma, the application of the hand to the hypochondriac regions generally produces distortions of the features, and complaints. The colour of the skin increases in intensity; petechiæ and extensive ecchymoses occur, along with hemorrhages from the nose, the vagina, the stomach and bowels, and bronchi. The urine contains brown bile-pigment and deposits a precipitate, containing amorphous mucus, coloured epithelium of the kidneys and urinary passages, and also needle-shaped crystals (tyrosine) coloured yellow. Sometimes there is albumen. Death usually occurs amidst increasing coma, and other symptoms of cerebral paralysis.

Above all things, as regards the symptomatology of this disease, it is important to bear in mind that "the extent of the hepatic dulness diminishes more and more, as the disease ad-

vances; and not unfrequently the dull space disappears completely, without there being any tympanitic distention of the bowels to account for it." The first three illustrative cases occurred to women in the seventh month of pregnancy. We are unable to give their details fully, but give an abstract of the necropsy of the second:—

On the 17th of January, 1858, she was attacked with symptoms resembling those of acute catarrh of the stomach; intense jaundice supervened; and on the evening of the 23rd, she died. The urine was dark-coloured, giving the reaction of bile-pigment, *but not of the biliary acids*. After standing, a greenish-yellow sediment was deposited, consisting entirely of acicular crystals of tyrosine, aggregated together in globular masses. When a drop of urine was evaporated, the residuum was found to consist of crystals of leucine and tyrosine, partially saturated with colouring-matter. At the necropsy, the skin was of a yellow colour. The large intestine contained scybala of fecal matter, but faintly tinged with bile. The mucous membrane of the bowel was everywhere pale and anemic. Ecchymoses existed in the mesentery. The spleen was enlarged by about one-third, soft, and of a pale red colour.

The liver lay collapsed against the posterior wall of the abdominal cavity; anteriorly, it was completely covered by the folds of the small and large intestine; it was dry and soft; its capsule was puckered and opaque, and its margin sharp. Its weight, in relation to that of the entire body, was only 1 to 68.5. In healthy females of the same age and weight, the ratio is 1 to 28. It was also considerably diminished in size: the left lobe measured 3 inches transversely, and  $5\frac{1}{4}$  from before backwards; and the right lobe,  $5\frac{1}{4}$  inches by  $5\frac{1}{4}$ ; the thickness was  $1\frac{1}{2}$  inch. The gall-bladder contained a small quantity of grey mucus. The tissue of the liver felt flabby, in some places it was congested; the ramifications of the portal vein surrounding the lobules were distended, whilst the centre of the lobules presented a citron-yellow colour. Here and there were small ecchymoses. A greyish-yellow substance was observed in the spaces between the lobules. In the left lobe, where the morbid process had advanced farther, this inter-lobular substance was still more extensive; the cut surface here presented an ochre-yellow colour, interspersed through which were distinct ramifying streaks. The secreting cells were completely disintegrated, and in their place were found numerous drops of oil and brownish-yellow molecules; it was only in the rounded border of the right lobe that a few isolated cells loaded with oil could be detected. The cut surface became covered with



a greyish-yellow film, consisting of globules of leucine mingled with crystals of tyrosine. Urea and phosphoric acid were absent from the urine. Its abnormal constituents have been already referred to. The blood in the heart, and in the venæ cavæ, contained a small quantity of leucine; and larger quantities were found in the cerebral substance, the liver and the spleen: these last organs contained a much larger amount than corresponded to what was found in the blood. The liver contained a considerable amount of tyrosine, whilst in the spleen this substance could not be detected with positive certainty,

The 18th observation is a case supplied to the author by Rühle—abdominal typhus, profuse epistaxis, violent delirium, jaundice on the fifth day, disappearance of the hepatic dulness, general muscular tremors, coma, death on the 8th day, are the conditions mentioned at the head of the case. The necropsic phenomena were small, shrivelled liver, with partially disintegrated cells, and empty bile-ducts, tumefaction of the spleen, deposits in Peyer's patches, and in the solitary glands of the ilium. But, as Frerichs observes, it is a matter of question whether this can be considered a case of typhus, complicated with jaundice, or of acute atrophy. It appears, indeed, often impossible to draw a sharp definition between the two conditions just named. The diffuse infiltration of the hepatic parenchyma, and the impaired secreting functions of the organ, which occur not unfrequently in severe cases of typhus, are abnormal states which differ from acute atrophy, with complete arrest of the hepatic functions, more in degree than in nature. In both cases, there is an exudation, which deranges the nutrition and the functions of the gland.

The author, after giving an account of the symptoms derived from the skin, the organs of circulation, the respiration, the organs of digestion, the urinary organs, and the nervous system, discusses the duration and mode of termination of the disease. Out of the thirty-one cases tabulated, the date of the commencement of three was unknown; in the remaining twenty-eight cases, the fatal termination occurred during the first week, 13 times; during the second week, 6 times; during the third week, 5 times; during the fourth week, 4 times.

A few cases have been recorded, as by Griffin and Hanlon (whose observations are familiar to us in the classic pages of Graves), where a favourable issue occurred; Budd also makes mention of such a case. But it may be made a matter of question, how far the diagnosis in these cases was perfectly correct. We can, at any rate, fully concur with the author, that "when

the anatomical lesions, from which this disease is thought to originate, have made considerable progress, and the larger portion of the hepatic cells have become disintegrated, one can understand that a cure can no longer be thought of." To the anatomical lesions we need no further refer, as the necropsy above given supplies us with the requisite data.

The nature of the disease is involved in much obscurity. It has been regarded as a process of bilious liquefaction (Rokitansky); as involving mainly a considerable impairment in the nutrition of the hepatic cells, and their subsequent disintegration by fatty metamorphosis (Henoch); paralysis of the bile-ducts has been also invoked (Von Dusch). These views are easily controverted; but it is more difficult to supplant them with a well-based hypothesis. Bright was of opinion that the disease essentially consisted in diffuse inflammation of the liver; and this view has obtained many supporters in Germany, of late, who have explained the destruction of the cells by a fatty degeneration, arising from an acute exudation-process. The author, though appreciating the difficulties which surround this doctrine, inasmuch as the familiar process of fatty degeneration offers no example of such rapid destruction of secreting structures, yet believes that an exudation-process constitutes the starting point of the disease—

"The bile-ducts at their origin are compressed at an early period, by the exudation at the periphery of the lobules; as a consequence of this, the secretion formed in the central portion of the lobules stagnates, and passes into the central veins, and so into the general mass of the blood."

An objection to this theory is alluded to by the author, but very summarily dismissed. We, on the contrary, think it fatal to this doctrine of exudation. It is, that no hyperemic enlargement of the organ can be detected during the progress of the disease. That the cell-structures of the organ break down under a certain acute destructive process, more or less allied to fatty degeneration, must be allowed; but of the steps preliminary to this process we absolutely know nothing, but must await further inquiry.

We must predicate, also, this last remark of the etiology of the affection, about which there is no satisfactory information. It is interesting to note that of the 31 cases, 22 were females; and of these 22, one-half were attacked during pregnancy. As regards diagnosis, the characters of the urine, bloody vomiting, jaundice, the remarkable nervous phenomena, and above all the diminution in size of the liver, must be chiefly relied upon;

but in the early stage of the disorder, a diagnosis is almost impossible.

The treatment consists of strong purgation, and, when there are severe pains in the liver, the application of leeches, cupping-glasses, and cold cloths. In full-blooded individuals, Frerichs advises venesection. Allusion is made to a recommendation of Dr. Corrigan, the administration of emetics. The hemorrhages require especial attention.

The author goes on to discuss disorganisation of the liver, induced by—1, Obstruction to the flow of bile, from impermeability of the great ducts; 2, By cirrhosis; and, 3, By fatty degeneration, properly so-called; and devotes the sixth chapter to the description of chronic atrophy.

This form of atrophy makes its appearance in connexion with various morbid conditions; but the author excludes those forms produced by the development of new growths in the liver, as cancer and echinococci, or having relation to cirrhotic wasting, or cicatrization. The author here includes only those forms of atrophy which are unconnected with any other obvious disorder of the organ, and may, therefore, lay claim to individual existence. In these forms the liver is usually of a brownish, or reddish-yellow tint, contracted in size; the lobular outlines very indistinct, or even annihilated; and the cells are small, angular in outline, having scanty or no granular contents, with intermixture of cells which have undergone fatty degeneration. The portal vein is usually considerably enlarged, as far as its subdivision into capillaries at the periphery of the lobules. The walls of the enlarged veins are sometimes normal, at other times they present a remarkable thickening of the sheath, formed by Glisson's capsule. The capillaries themselves are in a great measure destroyed; they become filled with brown molecules, or flakes, or granules of black pigment. The hepatic cells, too, often contain pigment granules.

These conditions of the liver are connected with very diverse states of the other abdominal organs, most of which, however, arise from chronic exudation-processes. The author's cases show ulcer of the stomach (No. 23); dysenteric cicatrices (No. 26); chronic dysentery (No. 27); fibrous thickening of the mesentery, with cicatrising ulcers in the small intestine (No. 28), to co-exist with, and apparently to originate, the morbid states of the liver. But other causes of great interest occasionally preside over the formation of this form of atrophy. Thus, in the twenty-fourth and twenty-fifth cases, the disordered nutrition of this organ supervened on intermit-

tents; and was, probably, directly caused by the blocking-up of the hepatic capillaries with pigment-granules. The twenty-ninth case displays the co-existence of atrophy with thrombus of the portal vein—itself secondary to a long-persistent thrombus of the pulmonary artery. The thirtieth case is also one of constriction of the portal vein, secondary to ulceration of the duodenum. The symptoms of this condition present very marked differences from those occurring in the acute disease, and lead us, inevitably, to the conclusion that the words acute and chronic give but a very imperfect idea of the total difference between these disorders. But this is a remark which has a far more general application than as refers to the matter in hand. Time offers, probably, but one of the elements which differentiate what pathologists are pleased to term acute and chronic diseases:—

“The destruction of a large portion of the hepatic parenchyma, in consequence of chronic atrophy, necessarily entails a diminution of the functional value of the gland, which reacts upon the system at large; and the more so, as a series of derangements in the digestive organs take place at the same time, owing to the obstruction of the portal circulation. In this way other functional complaints accompany atrophy of the liver, and make up the clinical features of this disease. The symptoms are developed slowly and insidiously. First, there are derangements of the gastric and intestinal digestion; loss of appetite, and a feeling of distention, and tightness at the epigastrium; tongue sometimes clean, and at other times furred; accumulations of flatus in the intestinal canal; and pale greyish-yellow or sometimes moderately brown stools. The bowels are irregular; constipation and diarrhoea alternate; persistent and profuse diarrhoea is frequently (in nine out of eighteen cases) observed, and soon induces exhaustion; in exceptional cases only are the bowels regular. On examining the hepatic region, the dimensions of the organ are found to be reduced in every direction; sometimes no dulness can be made out at all; in most cases, the organ is quite inaccessible to palpation. The spleen is usually unaltered; in a few cases only (in seven out of eighteen cases) is it enlarged.

“Symptoms of defective sanguification and nutrition supervene sooner or later upon these digestive derangements; the patients exhibit a pale cachectic appearance, without any jaundiced tinge; the muscular tissue wastes; and at the same time, as a general rule (in fourteen out of eighteen cases), accumulations of water take place in the peritoneal sac, which are soon followed by general dropsy. The urine is usually pale, and free from bile-pigment,—in a few cases it has presented a peculiar hyacinth-red colour; once only has it been observed to become dirty green, when treated with nitric acid.”

It need scarcely be added, that this disease terminates fatally; and the perusal of the author's admirable cases has indeed forced upon us the conviction that chronic atrophy is the final process by which death is brought about, in numerous abdominal diseases. One important observation, referring to diagnosis, we must allude to:—

“Simple atrophy can only be distinguished from that which arises from cirrhosis, when we can ascertain, by means of palpation, whether the surface of the gland is smooth or granular.”

The seventh chapter is upon “fatty liver,” upon which we are unable to tarry; and proceed to the next, in which the author gives an account of the pigment-liver.

This chapter is written with such marked ability and originality, that we are not surprised at the great attention which has been bestowed upon it by medical writers. The subject is, if not altogether new, at least one in which modern research has greatly added to the confused descriptions of the fathers of medicine. The so-called black bile constituted an important element of the earliest theories of humoral pathology. Galen thought that this substance accumulated in the spleen, as a product of the formation of bile, and that from that locality it gave rise to obstructions of the vessels, enlargements of the abdominal viscera, and dangerous nervous symptoms. We know how great a pathological importance was ascribed to atrabiliary matter by Boerhaave, and Van Swieten. During the first period of the rise of rational medicine, the doctrines of the ancients in this particular were cast aside, but the further progress of minute inquiry has nearly re-habilitated them. For “diseases became known in which black matter produced by decomposition of the blood became developed in the spleen, which black matter passed into the portal vein, and at one time obstructed the hepatic vessels, at another passed through these, and, entering the general circulation, filled up the capillaries of the brain and other organs—morbid processes, in fact, which gave rise to symptoms similar to those which were described by the ancients.” Many eminent observers have described a black change of various organs, originating, for the most part, from malarious fevers. Bright gives a drawing of a brain, the cortical substance of which was of a dark colour, “like black lead.” It was taken from a man who died of cerebral paralysis, after an attack of fever. It would appear that Meckel, in 1837, was one of the first to ascertain that the dark colour of the organ depended upon an accumulation of pigment in the

blood. A little later, Virchow found numerous pigment-cells in the blood, and in the enlarged spleen of a man who became dropsical after an intermittent fever.

In many individuals, who die from the effects of marsh-poison, the liver presents a steel-grey, or blackish, or not unfrequently a chocolate-colour. This change is produced by pigment-matter accumulated in the vascular apparatus of the gland. In fine sections such accumulation may be seen in the capillary net-work of the portal and hepatic veins; also in the larger branches. The deposits are either uniformly distributed, or in limited regions. Sometimes, owing to the interlobular veins being filled with coloured particles, the brownish-coloured lobules appear surrounded by black margins. In most cases, the arterial system is implicated; the hepatic cells, however, according to the author, remaining free from the dark deposit. As the disease advances, the liver diminishes in volume; and there frequently ensues a true atrophy, provided that the gland does not become infiltrated with colloid matter. Similar abnormal conditions are found in the spleen. When the pigment passes in large quantity into the circulation, the lungs become charged with it, and the brain assumes a remarkable tinge, its capillaries becoming filled with black granules and scales. The kidneys frequently participate in the pigmentary deposit, to a remarkable extent. Grey spots appear in the cortical substance; and, in a few cases, dark lines may be observed in the pyramids following the course of the blood-vessels and uriniferous tubes. The pigment may be detected in the capillaries of the cortical substance, and in the Malpighian bodies; occasionally isolated fragments are observed in the uriniferous tubes. The pigment in other organs and tissues is of subordinate importance. This dark material is brought to the various tissues by the blood. It consists of small rounded or angular granules, true pigment-cells,—some like the colourless corpuscles of the blood, others spindle, or club-shaped, with rounded nuclei. Larger fragments of pigment of irregular form are also observed, sometimes as large as  $\frac{1}{10}$ th of a line in length. The colour is, for the most part, deep-black, but it may be brown or ochre-coloured, or even reddish-yellow, representing the various stages of transformation of the red pigment of the blood into melanotic matter. Such are, in brief, the post-mortem appearances of melanemic individuals.

In reference to the place and mode of origin of pigment, most observers have agreed to place the seat of formation in the spleen; but, as experience demonstrates that these transformations can take place in any part of the vascular system,

or even external to it, we must pause before *limiting* their seat of production to that organ:—

“There can be no doubt, however, that the largest portion of the pigment is formed in the spleen; that it passes from this organ into the portal vein; and that part of it remains impacted in the liver, while the rest passes through these capillaries, and is carried into the general circulation.”

In melanemia, the accumulation of pigment is not so constant in any organ as in the spleen; next to the spleen, in order of frequency, comes the liver. The author has only met with one case where the pigment was present in the liver, while it was absent in the spleen. The question of its manner of formation is still unsolved. Stagnation of the blood in the abdominal glands is the only condition that can be at present predicated in reference to this question.

The effects upon the system of this morbid condition are most formidable; the blood becomes profoundly changed; the circulation in the liver seriously impeded; the bile is secreted in large quantity, and often contains albumen; leucine can always be detected in the hepatic parenchyma. Still more serious obstructions occur in the brain; hence, headache, noises in the ears, deafness, black vision, giddiness, delirium, convulsions, coma. But it is important to recollect that the cerebral disturbances of intermittent fever are not necessarily connected with melanemia: as might be expected, albuminuria is frequent, and complete suppression of the urinary secretion sometimes occurs:—

“The peculiar colour of the skin, which is produced by the quantity of pigment in the blood contained in the vessels of the cutis, is of importance in diagnosis. In the slighter grades of melanemia, it is ash-coloured; in the more intense forms, it is dirty greyish-brown, and sometimes deep yellow-brown. In most cases, a few drops of blood, obtained by scarification of the skin, suffice to show numerous particles of pigment under the microscope.”

These derangements are ushered in and accompanied by a fever, which may assume very various forms and types. In most cases, it is an intermittent, but the intermission is usually incomplete; and the three stadia of the paroxysm are seldom very distinctly marked, the rigor stage being often entirely absent. After two or three incomplete intermissions, the fever, in most cases, becomes continued. Some cases prove fatal in a few hours or days, while others are protracted over months.

The author distinguishes four forms: 1, Cases with pre-

dominant head-symptoms; 2, Cases in which the kidneys are pre-eminently implicated; 3, Cases with predominant derangement of the gastro-intestinal tract, and of the appertaining glands, but particularly of the liver; 4, Forms in which the local derangements are not very conspicuous, and characterized by anemia and hydremia, resulting from diseased spleen. There is a valuable collection of cases illustrating these various forms, to which we very earnestly invite attention.

The diagnosis can only be made with certainty by an examination of the blood,—the presence or absence of pigment in which, it must be our main object to determine. To the practised eye, “the peculiar grey, ash-coloured, or greyish-yellow colour of the skin, furnishes an obvious means of diagnosis.” The prognosis always remains a matter of doubt; coma and convulsions are, in general, of bad omen.

As soon as we are sure of our diagnosis, large doses of quinia must be administered; simple enlargements of the spleen usually yield to the use of quinia and steel. The congestion of the liver usually disappears spontaneously after the cessation of the fever, or even before this; when it remains stationary, rhubarb, extract of saponaria, with the neutral salts, the ammoniated sesquichloride of iron, with extract of aloes, are recommended. There is no known treatment for the atrophy of the organ. As regards the kidney complication, we find the author making the interesting remark, that the “albuminuria and hematuria which accompany the fever paroxysm, and remit and intermit along with it, *yield best to quinia*, and disappear, for the most part, so soon as the fever ceases.” When the abnormal condition persists, extract of cinchona, tannic or gallic acid, and the preparations of iron, are advised. The cerebral disturbances are to be treated on general principles, but we must not neglect the use of quinia.

The final chapter is on “Hyperemia of the liver, and its consequences,” to which we can now make no reference.

After this lengthened analysis, our readers will experience no difficulty in gauging the value of this monograph, which indeed presents to us a perfect model of medical research. Nothing can surpass the modesty of the author, but the admirable lucidity of his mode of treating the most difficult subjects. The translator, Dr. Murchison, has fulfilled his task so as to leave nothing to desire. He shows a thorough acquaintance with his author; and, both in his preface and in his most useful notes, has laid his readers under many obligations. We hope much from a physician who has displayed such *thoroughness* in all his undertakings, both as an investigator and as a *littérateur*.



*Cellular Pathology, as based upon Physiological and Pathological Histology.* Twenty Lectures delivered in the Pathological Institute of Berlin, during the months of February, March, and April, 1858. By RUDOLF VIRCHOW, Public Professor in ordinary of Pathological Anatomy, General Pathology, and Therapeutics in the University of Berlin, &c. Translated from the second edition of the original, by FRANK CHANCE, B. A., M. B., Cantab., &c. With notes and numerous emendations, principally from the MS. notes by the Author; and illustrated by 144 engravings on wood. London: Churchill. 1860. 8vo, pp. 511.

It would be a folly to deny that many of our ideas with respect to physiological knowledge are, in the present day, in an extremely unsettled condition; in fact, they may be said to be in that unsatisfactory transition state, in which no definite degree of precision has been arrived at, sufficient at least to command that full amount of confidence which a matter of such importance to the general interests of the human race ought, with every reasonable propriety, to be invested. Doctrines that had been advanced not many years ago as great and incontrovertible truths, and as such universally received, now no longer maintain that high character which then had been so readily conceded to them; and doubts which were at first only faintly whispered with respect to their ambiguous pretensions to public favour, gradually but surely have assumed a louder tone, quite sufficient, at all events, to cast a taint of suspicion on the soundness of the views entertained and propagated by those who were termed the disciples of the new school, whose theories, we will be candid enough to confess, were not always such as could be successfully defended; for they were too often the constructions of a hurried moment, and accordingly presented such a variety of weak points, that a skilful adversary had only to select his place of assault to succeed in his premeditated work of destruction. It was, indeed, hardly to be expected that new and startling doctrines, whose avowed object was to overthrow others that for centuries had been looked up to with reverence and respect, should be exempt from the attacks of the most searching and merciless criticism; and hence became necessary the greater care and caution, to shield with the most consummate skill every avenue of approach of which a watchful assailant might be led to take advantage. Accordingly, no hasty or half-formed conclusion should have been admitted,—no view advanced, that would not bear the strictest and closest ana-

lysis—nothing stated as a fact, that could not be as readily proved to be such; while every sentence, as it was committed to the ordeal of public opinion, should have been so constructed as to endure without flinching the scrutiny which, from its very nature, it seemed to court and defy.

It was not by a series of rapid and unreflecting strides, such as we often are compelled to witness in the present day, that our forefathers wrought out those numerous and truly wonderful discoveries, apparently in the teeth of the most insurmountable difficulties: the sole compensating power which enabled them to succeed in achieving their object, evidently consisting in an untiring patience and steady perseverance, aided by a peculiar concentration of mind, for the time being, on the object of their pursuit. They sifted the subject of inquiry to the very dregs, and left but little to be added by him who followed in their foot-prints, until the rapid progress of modern science placed in the hands of the present generation a multitude of mechanical appliances that were intended evidently to aid, not to supersede, the functions of the intellect. That such a result has not, however, been produced, it would be in vain to deny. We have allowed our better judgment to be led captive by an innovation that as yet wants a firm foundation to rest upon; and hence, those many quicksands that are so continually and treacherously shifting their ground beneath our feet, leaving us too often in a state of extreme peril, uncertainty, and embarrassment.

In former days men were anxious to obtain a distinctive character for the depth of thought, and power of reflection, which they were able to bring to bear on any given subject; and this, in fact, was the scale by which their capacity was universally measured. At present, however, we estimate a man's understanding by no such sensible rule, but foolishly suffer ourselves to be led away by a certain quickness and rapidity of idea, which, when thoroughly examined, is found to be a shadow, and nothing else. The truth would seem to be, that we have no time now for that sober and tedious process of reflection, in the true sense of the word; for our aim is to be considered as an universal genius; and while we thus permit ourselves to dabble in such a multiplicity of subjects, we must as naturally fail in acquiring what might be termed a respectable knowledge in any one of them. The period of our life is by far too short to allow us to indulge in any such visionary expectation of attaining universal excellence; and hence, with many companions in our folly, it is true, we fritter away our

intellect in a pursuit after an ephemeral fame, and sink into the silence of our graves at last, leaving nothing whatever behind us to indicate the position that we once occupied in the world, but a momentary void, which will be almost as immediately filled by some other votary, as anxious to sacrifice at the same shrine of insensate folly.

It is men of this stamp more than any other who are hourly guilty of the most flagrant treason against the true progress of science. They do it, in fact, much more real injury than its most open and avowed opponents; for their ill-directed attempts, illogical reasoning, and lame and impotent hypotheses—for they confess with the greatest candour, that from the multitude of their other avocations, they cannot command sufficient time to handle the subject as its importance might deserve—are positively destructive to a cause, which, in the spirit of charity, we must suppose it was their most earnest wish to advocate and uphold. Did we require a striking example of the truth of what we have just stated, we have only to direct our attention to the scenes that have occurred in our courts of law within the last few years. The lofty pretensions which chemistry once laid claim to,—where are they now? Are they not gone, vanished, and that we fear irrevocably? Are they, we would ask, now considered of the value of a feather's weight in turning the scales of justice one way or the other? Are they not regarded by legal authorities as all but utterly worthless—or, to speak more properly, do they not look upon its noblest deductions merely as an article that is for sale, to be prostituted at any moment to the vilest purposes, as the occasion may require? Dare we assert that this decision, unfounded though it be, in positive fact, has not been arrived at on what may be deemed just and substantial grounds? For it cannot be expected that men totally unacquainted with this particular branch of science will take the trouble of endeavouring to reconcile the apparent contradictions that absolutely do exist—they will look to the fact, and the fact alone; and will as naturally ascribe to the erroneous deductions of chemical science, what ought with greater justice be imputed to the ignorance and want of skill of the contemptible smatterer—the would-be-universal genius, whose vanity had led him to suppose that he was capable of using the means at his disposal with the tact and precision of a master in his craft. Try now, when the mischief has been completed, to convince this wretched pretender of the dark obloquy that he has cast on this, the noblest of all sciences. Show him as clearly as the noonday sun, that his reasonings were all so

many fallacies, his views erroneous, and his statements not worth the breath that was expended in delivering them. Think you that you will draw a murmur of regret for what has been done from that mass of animated egotism, that listens with a quiet sneer on his countenance to all that you can urge in your wrath on this occasion. The truth is, that he is at this very instant waiting impatiently for the moment that will enable him to make a similar display of his stock of erudition—limited in one sense certainly, but as truly multifarious in another—on a branch of science the very opposite to the one in which he has so lately shone with lustre so peculiarly his own.

But chemistry has not, however, the honour of standing alone as an example of the discredit that may be brought on a particular pursuit by the errors that must, of necessity, be the offspring of assuming folly, and still more of presumptuous pretension. Let us direct our attention for one moment to the microscope, and see how low it has fallen in general estimation, owing unquestionably to the awkward attempts of a few shallow pretenders, striving to attain a character for proficiency in what, even in the hands of the most expert in the art, must always be beset with a host of difficulties, which nothing but time, patience, and a continuously uninterrupted practice can ever hope to overcome. True it is, that we have always felt, and never took the trouble of concealing this feeling, that the powers of this wonderful instrument of man's ingenuity, constituted as it is at present, have been greatly overrated, and more has been always expected from it than it had the power to accomplish; still, with so many and so important revelations effected by its agency, yet vivid in our memories,—with the explanation of several mysterious facts, hitherto inscrutable, accomplished by the same means, still fresh in the mind's eye,—it would be unjust, as well as unwise, to attribute to the above-mentioned cause alone its gradual declension in public estimation. Take even the most trivial affair in ordinary life, let a diversity of opinion exist, and doubt will assuredly follow. In appealing to one of the most trustworthy of our senses, the microscope did, at first, most certainly acquire a confidence almost unlimited; and no matter how opposed its revelations might be to all our former preconceived impressions, we were still satisfied to receive them with respect, and sacrifice even our own most favourite opinions to its more manifest enunciations. And now arose a host of professional amateurs, each with instrument in hand, and all anxious to parade their limited stock of knowledge before the public gaze,—eager to acquire a character for originality of idea; and unhesi-

tatingly publishing a series of pretended discoveries to the world, that could, in truth, boast no firmer basis of support than the wild and visionary dreams of their own excited imaginations. The theories which they had the folly to advance fell, of course, beneath the storm of criticism with which they were universally assailed; and in their fall, exactly as in the case of its twin-sister, chemistry, barbed the shaft which every fool was ready to seize upon, and hurl against the rational supporter of a branch of science that had failed simply from being abused by those who wanted the intellect to employ its acknowledged powers for the purposes for which it was really destined.

Strange it is, but nevertheless true, that example is utterly thrown away on individuals such as we have alluded to. The first themselves to inveigh with merciless rigour against the error of another—the foremost to point out, and that sometimes with much propriety the irreparable injury that is inflicted on science by crude and ill-judged exhibitions of this nature,—they are themselves equally hasty in falling into the same pitfall, to meet with a fate precisely similar to that which they had been so ready to award to those who had preceded them. The punishment, however, will always fall lightly on men of this stamp; for such is the amount of their overweening egotism and self-esteem, that they will always experience a certain degree of consolation in the reflection, that their want of success ought to be attributed, not to any deficiency on their own part, but rather to the injustice of a jealous world, that sees clearly enough, but refuses to recognise, their high and legitimate claims to well-merited consideration.

We dwell thus strongly on this subject, because we perceive that it is an evil which is latterly acquiring fresh strength; and, if allowed to proceed with the same railroad rapidity, it is difficult to say to what a climax it may yet arrive. Boys hot from the dissecting-room,—whose powers of reflection, from the very nature of the avocation they have been obliged to pursue for so long a period, must be too limited to demand even a passing notice—whose efforts at deduction are too puerile to excite even a compassionate laugh—whose cheek is still graced by the modest down that unkindly refuses to the youthful investigator a single hair wherewithal to deck his object-glass—whose attention never was concentrated on any one single object for five minutes consecutively in their life—do not these embryonic representatives of incipient manhood, in form we mean, not brain, presume to lisp out their doubts of some well-established theory, and in the same breath have the audacity to suggest one of their own as being more rational, and

better calculated to elucidate a grave and intricate subject? Aye, and bearded men, too, whose maturer years ought to argue a fuller amount of wisdom, are not ashamed to follow in the same track, but vie with those pseudo-philosophers in their egregious folly,—pursuing, however, a rather more ambitious flight; for their eloquent disquisitions are confined to “cytoblasts, blastemas, plasmas,” &c. &c., though the chances are, that there are not more than five out of every hundred that clearly understand the accurate meaning of those technical terms that they are everlastingly, and on every imaginable occasion, forcing into the most ordinary conversation. Nor will they hesitate to volunteer a florid and elaborate description of a particular tissue in a state of disease, though utterly and hopelessly ignorant of the character it presents in a condition of health. In short, they appear to take an insane delight in the effort of exposing their own incorrigible ignorance, quite regardless of the heavy tax they thus impose on the patience and courtesy of their unfortunate auditors, who are compelled to listen in silence to their incessant jargon of gross and incomprehensible absurdities.

In one respect, at least, we may lay claim to a close affinity to the monkey tribe—we mean, in the habit of imitation—not only in small things, but also in great. To conceal a bodily defect, which we instinctively feel will have a tendency to compromise the position which we are anxious to hold in the eyes of the world at large, vanity will not be slow in fabricating a certain style of attire which good sense, in every other way strictly unimpeachable, will as certainly, after a little time, completely lapse into. “Out of the fashion, out of the world,” is an old but true remark; and so thoroughly are we convinced that it holds its ground, even to the present day, that we do not entertain the least doubt that, if Sir David Brewster would take it into his head to revert to the amusements of his boyish days, by placing a gold-headed cane between his legs, and so parade the public streets, not one, but a thousand, pseudo-philosophers would be at once similarly mounted, capering and prancing along our pathways, proud of the appearance that they made, and as eagerly seeking for the admiration of every casual passer-by. And so it is with the microscopic mania, which is at present raging like some fierce epidemic—the staid footsteps of wisdom hurrying on in the track that has been imprinted but a few moments before by the lighter tread of folly. Nay, to such a pass has the delusion arrived, that even the busy practitioner—engaged from morning till night in the harassing pursuits of his profession, working literally for his mo-

dicum of daily bread, and the blanket to stretch over his wearied limbs during the midnight hours, with a mind in one continuous vortex of anxiety and disquietude—he, too, if he wishes to hold his ground amongst his brethren, must succumb to the prevailing fashion, and write in living characters his adhesion to a system so contemptibly ridiculous—we say, ridiculous, for surely no rational man can imagine for a moment that the few brief moments that he can snatch from his more active and imperative avocations can, by less than a miracle, be sufficient to unveil to the peculiar quickness of his vision what it would take the professed microscopist weeks, nay, perhaps, months, to unravel satisfactorily. And yet, that such are the melancholy spectacles we are doomed to behold day after day, no one who has devoted the least attention to the subject will presume to deny; and the only excuse that can be urged in extenuation of it is, that it is, unfortunately, the fashion of the times we live in, and, under the circumstances, we are in a manner coerced to keep up to the mark; as a friend of ours once remarked to us, when borrowing a book on microscopic anatomy, to enable him to talk with some degree of fluency on this intricate subject, while, at the same time, he as naively confessed that he had never looked through the tube of a microscope in his life.

“*Desipere dulce est in loco*,” so spoke the most sensible and acute of all the ancient satirists; but we, it would appear, have improved wonderfully on this axiom; for, in season and out of season, we display no symptom of shame in mounting our favourite hobby, and courting a notoriety which it would have shown much more real sense to have avoided altogether. One fact, with respect to the subject under discussion, has often struck us as being very remarkable, and that is, the gross ignorance which is so universally displayed by the profession at large regarding the amount of time and labour that must be expended in microscopic investigations, in connexion with pathology, if the object intended is really to derive any positive advantage from them; and here is another reason why so many shallow pretenders are so continually springing up, while the numbers of the real proficient in the art are comparatively so few. It was in order to correct this evil we have heard that a gentleman, holding a distinguished and well-deserved position in public estimation for years, the physician to one of our largest metropolitan hospitals, has suggested the appointment of a regular microscopical pathologist to each of those invaluable charitable institutions, under the impression that such an individual, acting as a species of accre-

dited *sensor morum*, would, in a great degree, tend to modify and control many of those unnatural vagaries that are, at the present moment, poisoning the very life-blood of an incipient branch of science, which, if properly handled, promises to do so much, in time to come, for the cause of humanity. It is needless for us to attempt to describe the pleasure it would afford us, were it possible that the proposed scheme could be thoroughly carried out; but, apart from other difficulties, which are too obvious even to be alluded to, we will merely point to one, which is so tangible in its nature, that it must not be for a moment overlooked. Where are the funds to come from sufficient to compensate the man properly qualified to undertake, and as well calculated to discharge efficiently the duties of such an office, more especially when it is recollected that it is the most valuable hours of his time that will be required from him, aye, and exacted, too, to the last moment? In these degenerate days, the honour of such an appointment will count but little indeed; for, as the rule, the more gifted of mankind are beginning gradually to acquire that amount of wisdom that leads them to decline a gratuitous offer of their brains for fools to banquet upon; for they have at length discovered that, whilst luxuriously indulging their appetite, they are by no means backward in insinuating that it is to them alone (the fools we mean) is due the sole honour and credit of providing the materials for this magnificent feast.

The man of genius, too long a child in simplicity of character, as far as worldly matters are concerned, is at length slowly becoming aware of the precise standard value that ought to be attached to the patronising nod, and ready shake of the hand, with which he may be greeted, according to the current of circumstances. Beneath this specious garb he is instinctively beginning to learn to recognise the identity of the confirmed brain-sucker, who, battenning upon the rich intellect of his fellow-man with complacent simper of satisfaction on his face, lisps out his pirated stock of information to the world, with an unblushing assurance, that an honest man might well envy and strive to imitate, but in vain, in a better cause. Not a century has elapsed since an individual, with what was then termed the most unjustifiable effrontery, had the boldness to coin out a character for himself from the posthumous ore committed to his charge by a dear friend, and still closer connexion. The voice of public opinion was deservedly raised against this deliberate act of literary chicanery; and the withering scorn of all that was good, all that was honest and upright, was poured in scathing torrents on his devoted head; and yet he had, at least,



the decency to bide his time, and wait until the true owner of what he had so surreptitiously appropriated to his own use had lost the privilege of claiming, with a living voice, what really did belong to him as his own undoubted property. Do the jackals of literature of the present day observe a similar decency in their conduct, and pause until the hand of death has quenched the voice of their victim in everlasting silence? They do no such thing, but filch, and that openly, from the breathing man—aye, and so would they act towards the unfortunate pathological microscopist who would suffer himself to be lured into such a position solely by the imaginary *eclat* attending on such an appointment; for soon, very soon, he would be taught to feel, that while his was all the trouble, toil, and loss of time, others, and those whom he dared not disoblige, would as certainly spring up to lay claim to all the honour and credit that might be the result of his labours and exertions.

We have spoken thus openly, candidly, and perhaps a little strongly, on this subject; for this fashion of pretence and assumed knowledge of abstruse science is becoming absolutely intolerable to every well-regulated mind. Still, we would not wish it to be understood for one moment that we have the slightest intention to find fault with, or decry, what may be commonly termed general microscopic investigations; for in this way we frequently dabble in it ourselves, taking, however, right good care to keep all our imaginary discoveries within the precincts of our own breast, and not foolishly force them on the unwilling ear of an incredulous race, who are always slow in appreciating properly the merits of aspiring talent. We would, therefore, most earnestly advise passionate amateurs in this fascinating amusement—for such always it must be in their hands—to rest satisfied with such specimens as they can procure in abundance from the dealers in such articles, which will at least have this one positive advantage—that of saving them the trouble of amassing a number of nondescript preparations, which, but for the reference on the margin, would never be recognised for what they were intended to represent. Or, again, can they not with good plates by their sides, endeavour to obtain a view of some natural object? And no matter how faint the resemblance may be to the picture before them, surely a glowing imagination, that does not halt at trifles, will be fully competent to fill up many a gaping void, and confer a graceful harmony on the whole exhibition.

All this can do no harm—in fact, it is attended with the positive good of affording a certain relief to the over-wrought

mind, and restoring it to the proper tone for carrying out the more sober details of every-day life; while it as assuredly ceases to be a recreation, and is converted into a positive nuisance, the moment it emerges beyond the privacy of the study, and stalks conspicuously before the eye of the world.

We shall close our homily with the following apt illustration of what we have been attempting, we fear uselessly, however, to impress:—one of the most illustrious statesmen that ever directed and controlled the mighty destinies of Great Britain, was one day detected, when he conceived himself quite free from observation, on hands and knees on the carpet, with a child mounted on his back, to whom he enacted the part of a docile and delighted steed. The feeling that prompted the great and gigantic intellect to descend to such a boyish pastime was one that, under the circumstances, all must admire; for it was a father's heart that induced him to the act, and led him to feel a sympathetic pleasure in the delight which he was thus conferring on the joyous and exulting boy. Photograph him now in this strange position, and suspend him in every print-shop in the metropolis. What is the result? The picture which the most morose ascetic might once have gazed on without, at least, a feeling of displeasure, has now become so positively ludicrous, that even the fool may be pardoned the broader grin with which he salutes it whenever it chances to meet his eye.

In the work of the great and acknowledged German Professor at present lying before us, curiously enough, we find the following passage, corroborating strongly the opinions which have been advanced by us a few sentences back. Speaking of the limited opportunities enjoyed by the busy practitioner for pursuits of this nature, and his want of sufficient time to carry out his investigations effectually, he thus proceeds to say:—

“And for this reason, that I thought I must take it for granted that many busily occupied physicians were not quite familiar with the most recent histological changes, and did not enjoy sufficiently frequent opportunities of examining microscopical objects for themselves. Inasmuch as, however, it is upon such examinations that the most important conclusions are grounded which we now draw, you will pardon me if, disregarding those among you who have a perfect acquaintance with the subject, I behave just as if you all were not completely familiar with the requisite preliminary knowledge.”

This soft language, by the bye, is only a happy art of clothing a rather disagreeable subject in a delicate style of words; for we may rest assured that the land of the learned

professor is just as rife in pretension as our own native soil, a fact of which none was better aware at the moment than the speaker himself. Preliminary knowledge, indeed! What a cutting sarcasm these two simple words must have conveyed to the unfortunate auditors that were seated before him; for, with every wish to give them credit as well-educated and thoroughly informed physicians, we are yet strongly inclined to believe that their insight into the elements or principles on which pathology must of a necessity be based, might be measured by the minutest of all homœopathic scales, without the least packing. Nor is he a particle less indulgent a little farther on, where he makes the following remark with respect to the amount of light that is required for microscopic purposes, with the deceptive results that will be produced by its excess,—an observation that might still be farther, and with great justice, extended and applied to several other abuses in the manipulation of this delicate instrument, by which the most absurd and ridiculous effects are frequently produced,—tending unquestionably to confirm an opinion religiously believed, and advocated by an ingenious friend of ours, who upholds, and is not ashamed to defend the doctrine, that all the appearances which we see, or pretend to see, with the high powers we at present possess, are nothing more than minute particles,—the constant constituents of the atmosphere—and that it depends simply on the accidental turn of the movement wheel, which of them may by chance be brought into view. He may be right, or he may be wrong, for aught we know to the contrary, in this age of progress; but the words of the Professor show that something like this may sometimes take place:—

“In the course of the last ten years of the last century there arose, however, a certain degree of reaction against this fibre-theory; and in the school of natural philosophers another element soon attained to honour, though it had its origin in far more speculative views than the former, namely, the *globule*. Whilst some still clung to their fibres, others, as in more recent times Milne-Edwards, thought fit to go so far as to suppose the fibres, in their turn, to be made up of globules ranged in lines. This view was in part attributable to optical illusions in microscopical observation. The objectionable method which prevailed during the whole of the last and a part of the present century—of making observations (with but indifferent instruments) in the full glare of the sun—caused a certain amount of dispersion of light in nearly all microscopical objects, and the impression communicated to the observer was, that he saw nothing else than globules. On the other hand, however, this view corresponded with the ideas common amongst

natural philosophers as to the primary origin of everything endowed with form."

In glancing our eyes over the pages before us, one article, that on the nails, has struck us as being so eminently beautiful, and apparently so true in all its details, that we regard it as a real gem in this casket of science. It follows gracefully the section on the skin; the latter presenting no new feature worthy of remark, and only confirming what we ourselves have always been in the habit of teaching, that the so-called *rete mucosum* is nothing more or less than the deeper and more highly organized layers of the epidermis, and not a distinct structure, as is so generally supposed. Our views, however, with respect to the development and structure of the nail were not of a character so definite and precise; and we were accordingly delighted to receive a lesson on this subject from one who, from his position and language, can be safely relied on. His remarks on its pathological condition are likewise curious, and such as are not unfrequently witnessed in the career of general practice:—

"Very much the same is the case with the nails. On examining the section of a nail, made transversely to the long axis of the finger, we see virtually the same structure as in ordinary skin, only every single indentation of the inferior surface does not correspond to a conical prolongation of the cutis, or papilla, but to a ridge which runs along the entire length of the bed of the nail, and may be compared with the ridges which are to be seen upon the palmar surface of the fingers. Upon these ridges of the bed of the nail are dwarfish, stunted papillæ, and upon them rests the rather cylindrically shaped youngest layer of the rete Malpighii; then follow cells continually increasing in size, until at last the really hard substance comes, which corresponds to the epidermis.

"Nevertheless—to discuss the subject at once, seeing that I shall not again have occasion to mention it—the structure of the nails has been difficult to make out, because they were conceived to be a simple formation. Nearly all the discussions, therefore, which have taken place, have turned upon the question where the matrix of the nail was, and whether the growth of the latter took place from the whole surface or from the little fold into which it is received behind. If we consider the nail with respect to its proper firm substance, its compact *body* (Nagelblatt), this only grows from behind, and is pushed forward over the surface of the so-called *bed of the nail* (Nagelbett), but this in its turn also produces a definite quantity of cellular elements, which are to be regarded as the equivalents of an epidermic layer. On making a section through the middle of a nail, we come, most externally, to the layer of nail which has grown from behind, next to the substance which has

been secreted by the bed of the nail, then to the rete Malpighii, and lastly to the ridges upon which the nail rests.

"Thus the nail lies in a certain measure loose, and can easily move forwards, pushing itself over a moveable substratum, whilst it is kept in place by the ridges with which its bed is beset. When a section is made transversely through a nail, we see, as already mentioned, essentially the same appearance presented as that offered by the skin, only that a long ridge corresponds to every single papilla seen in ordinary sections of the skin; the undermost part of the nail has slight indentations corresponding to these ridges, so that, while gliding along over them, it can execute lateral movements only within certain limits. In this manner, the body of the nail which grows from behind moves forward over a cushion of loose epidermic substance in grooves which are provided by the ridges and furrows of the bed of the nail. The uppermost part of the nail, if examined when fresh, is composed of so dense a substance that it is scarcely possible to distinguish individual cells in it without applying reagents, and at many points an appearance is presented like that which we see in cartilage. But by treating it with potash, we can convince ourselves that this substance is composed of nothing but epidermis cells. From this mode of development, you will see how easily intelligible distinctions may be drawn between the different diseases of the nails.

"There are diseases of the bed of the nail which do not affect the growth of its body, but may give rise to changes in its position. When there is a very abundant development of cells in the bed of the nail, the body may be pushed upwards—nay, it sometimes happens that the nail, instead of growing horizontally, shoots perpendicularly upwards, the space underneath being filled with a thick accumulation of the loose cushiony substance (*Polstermasse*). Thus suppuration may take place in the bed of the nail without the development of its body being thereby impeded. The most singular changes occur in small-pox. When a pock forms upon the bed of the nail, there is nothing to be seen but a yellowish, somewhat uneven spot; but if, on the other hand, it is developed upon the fold, then its traces are left in the shape of a circularly depressed, and, as it were, excavated spot in the body of the nail as it gradually advances, a proof of a loss of substance precisely similar to that which takes place in the epidermis."

We would most strongly recommend the perusal of the article on muscle and muscular fibre to our readers; while we can only express our regret that from its great length, and the consecutive reasoning with which it is treated, we find it impossible to make such extracts as would convey a just idea of it as a whole. For this much, however, we can safely vouch, that it gives a truly excellent exposition of this much-debated subject, superior to any we have as yet met with, and open

to far fewer objections than can be urged against any of those that have hitherto preceded it. The same observations may be applied, and with equal justice, to the description of the development and ultimate formation of bone and its precursor, cartilage. So simple and so clear is the author's language on this intricate subject, that we can follow him without an effort; and find no difficulty, with the assistance of the admirable illustrations, in bringing before the mind's eye the vivid representation of all that he is so anxious to convey. There are, however, in the last paragraph of the fourth lecture, several things so new, and which may yet exercise a very marked influence on an extremely dangerous and stubborn disease, that we are induced to extract it entire for the benefit of the reader.

“These systems of tubes which are found in such a very marked form in bone and the teeth, are to be seen with far less distinctness in the soft structures; and it is chiefly for this reason, I imagine, that the analogy which exists between the soft connective tissues and the hard texture of bone has not been clearly comprehended. These systems are most distinctly seen in parts which are more of a cartilaginous nature, as, for example, in fibro-cartilage. But it is a fact of great significance that we find a series of transitional forms between cartilage and the other connective tissues, in which the same conditions are constantly repeated. In the first place, parts which chemically belong to the class of cartilages, for example, the cornea which yields chondrine when boiled, although nobody regards it as real cartilage. But more striking is the arrangement in those parts in which the external appearance speaks in favour of a cartilaginous nature, but the chemical properties do not correspond, as, for example, in the semi-lunar cartilages (*Bandscheiben*) of the knee-joint, which are interposed between the femur and tibia for the purpose of protecting the articular cartilage from too violent contact. These parts, which even now are generally described as cartilage, yield no chondrine on boiling, but gelatine; and yet in this hard connective tissue, we meet with the same system of anastomosing corpuscles that prevails in the cornea and in fibro-cartilage, and it is displayed with unusual distinctness and clearness. Vessels are almost entirely wanting in these cartilages, but in exchange they contain a system of tubes of rare beauty. On making a section, we see that the whole is in the first place mapped out into large divisions, exactly like a tendon; these are subdivided into smaller ones, and these are pervaded by a fine, stellate system of tubes, or, if you will, of cells, inasmuch as the notion of a tube and that of a cell here quite coincide. The networks of cells which here form the system of tubes, terminate externally in the septa bounding the individual divisions, and we here see in close proximity considerable collections of spindle-shaped cells. In these cartilages, too, the whole mass of tissue is only connected by its exterior with

the circulatory system; everything that penetrates into the interior must pass by a very circuitous route through a system of canals with numerous anastomoses, and the nutrition of the internal parts is altogether dependent upon this mode of conveyance. The semi-lunar cartilages are structures of considerable extent and great density, and, as they are entirely dependent for their nutrition upon this ultimate, minute system of cells, we have in them, much more than in cartilage, to deal with such an arrangement for the supply of nutritive juices, as cannot be under the direct control of the vessels.

"For the sake of elucidation, I will merely add that the ultimate elements are seen to consist of very delicate cells, which are prolonged into fine filaments, that in their turn ramify, and look, when cut across, like small points in which a clear centre can be recognised. The filaments can ultimately be very distinctly traced back to the common cell, just as in bone. They are extremely fine tubes which are intimately connected with one another, only that here they are in certain spots collected into large groups, by means of which the conveyance of the nutritive juice is principally effected, and that the intercellular substance in no instance becomes infiltrated with lime, but always preserves its character as connective tissue."

In the space to which we are compelled to confine ourselves, it is utterly impossible to notice, as we would wish, many subjects of a character strikingly novel, and certainly very much at variance with the opinions hitherto looked upon as firmly established in every respect. In this category we might class the several articles on the blood, lymph, &c., with the changes that so frequently are well known to occur in them, constituting a variety of diseases that are recognised under the name of pyemia, leuckemia, phlebitis, emboli, &c., &c. Every well-informed physician is, of course, perfectly aware of the symptoms that are usually found to identify each of these several lesions, and which will occasionally start up in his path in the course of his practice, but we are much inclined to doubt whether he as accurately understands the true pathological principles of their nature. To such as are naturally inquisitive in such matters—and what properly educated man would confess the reverse?—we would most earnestly recommend the perusal of the several lectures devoted to these topics, which he will find handled in a style so masterly and so explicit, and yet so free from every vestige of narrow-minded pedantry, that we are convinced he will feel exactly as we did when we had terminated, astonished that the simple views here laid down had not previously occurred to the mind of any preceding pathologist. And herein we conceive lies the striking excellence of the

whole work, that we are not called on to believe what is contrary to our senses, opposed to our reason, and at variance with our better judgment—on the contrary, we are rather invited to call into operation the functions of our own thinking power, the gifted lecturer himself merely suggesting the subject for thought, and leading us insensibly on, until he has conducted us to the climax, almost imperceptibly, indeed, which he had long before anticipated.

We will close our extracts from this volume by one from the last lecture, which, as giving the opinions of the author, with respect to the microscopical and pathological nature of tubercle, is deserving of an especial notice; for who has not had reason to mourn from the ravages of this inexplicable disease? and where the physician, so proud of his own attainments, who has not felt the blush of shame mantling his cheek, as the slow and insidious step of this deadly affection stole silently, but slowly, on, and he was powerless in checking its onward progress? Before this unsparing and relentless foe, that mocks its intended victim, while its remorseless gripe is gradually tightening on the well-springs of life, how lowly we fall in our own estimation, wringing from us the sincere, but bitter confession that, in this case, all our art and all our knowledge stand forth like pigmy dwarfs against such a gigantic and invincible antagonist. The fate of the French medical student, who, made aware of this incurable affection under which he was labouring, dared to anticipate by more speedy means the event which he knew must be his lot, though by a process slower, indeed, but equally as sure, has branded in characters that never can be effaced our utter impotency to deal with this destructive malady successfully in the present condition of our knowledge. We ought, therefore, to hail as it deserves, every attempt that may be made to give us a proper insight into this hitherto unfathomable secret, as it may thus conduct us perhaps to a more rational mode of encountering an enemy who has hitherto proved so totally unassailable in every respect:—

“It may, at least according to what I consider to be the correct view of the matter, certainly be said, that the greatest part of whatever in the course of tuberculosis does not appear in the form of granules, is an inspissated inflammatory product, and has at any rate no direct relation to tubercle. But by the side of these inflammatory products, or also independently of them, we find a peculiar structure [the knot, granule] which, if they are to be regarded as real tubercle, would no longer be included in their ordinary classification; and it is certainly an extremely characteristic circumstance that in France, where the terminology of Lebert has become the pre-



vailing one, and the *corpuscules tuberculeux* are wont to be regarded as the necessary accompaniments of tuberculosis—bodies, concerning the tuberculous nature of which there can be no doubt, have quite recently been set down as something altogether peculiar, and which had hitherto remained undescribed. For one of the best, nay, perhaps the best, micrographer France possesses, Robin, has, in his examinations of cases of tubercular meningitis, deemed it impossible to regard the little granules in the arachnoid [pia mater] which everybody looks upon as tubercles, as being really tubercles, because the dogma now prevails in France that the tubercle consists of solid non-cellular corpuscles, and in the tubercles of the cerebral membrane cells in a state of perfect preservation, are met with. To such curious aberrations does this track lead, that one ends by being unable to find a name for real tubercle, because so many accidental objects have been confounded with it, that what was sought for, or even what had been found and was already grasped, has in consequence of the attention of observers being diverted by these objects, been allowed to slip out of one's hand again. I am of opinion that a tubercle is a granule, or a knot, and that this knot constitutes a new formation, and indeed one which, from the time of its earliest development, is necessarily of a cellular nature, and, generally, just like all other new formations, has its origin in connective tissue, and which, when it has reached a certain degree of development, constitutes a minute knot within this tissue, that, when it is at the surface, projects in the form of a little protuberance, and consists throughout its whole mass of small uni- or multi-nuclear cells. What especially characterizes this formation is the circumstance, that it is extremely rich in nuclei, so that when it is examined as it lies imbedded in the tissue which invests it, at the first glance there seems to be scarcely anything else than nuclei. But upon isolating the constituents of the mass, either very small cells provided with one nucleus are obtained—and these are often so small that the membrane closely invests the nucleus—or larger cells with a manifold division of the nuclei, so that from twelve to twenty-four or thirty are contained in one cell, in which case, however, the nuclei are always small, and have a homogeneous and somewhat shining appearance.

“This structure which in its development is comparatively most nearly related to pus, inasmuch as it has the smallest nuclei and relatively the smallest cells, is distinguished from all the more highly organized forms of cancer, cancroïd and sarcoma, by the circumstance, that these contain large, voluminous, nay often gigantic corpuscles with highly developed nuclei and nucleoli. Tubercle, on the contrary, is always a pitiful production, a new-formation from its very outset miserable. From its very commencement it is, like other new-formations, not unfrequently pervaded by vessels, but when it enlarges, its many little cells throng so closely together, that the vessels gradually become completely impervious and only the larger ones, which merely traverse the tubercle, remain

intact. Generally fatty degeneration sets in very early in the centre of the knot (granule), where the oldest cells lie, but usually does not become complete. Then every trace of fluid disappears, the corpuscles begin to shrivel, the centre becomes yellow and opaque, and a yellowish spot is seen in the middle of the grey translucent granule. This is the commencement of the *cheesy metamorphosis* which subsequently characterizes the tubercle. This change advances from cell to cell farther and farther outwards, and it not unfrequently happens that the whole granule is gradually involved in it.

"Now, the reason why I think that the name of tubercle must be specially retained for this formation, as being extremely characteristic of it, is this—that the tubercle-granule never attains any considerable size, and that a tuber never arises out of it. Those which are wont to be termed large tubercles, and attain the size of a walnut, or a Borsdorf apple, as for example in the brain—those are not simple tubercles. You will generally find the tubercles in the brain described as being solitary, but they are not simple bodies; every such mass (tuber) which is as large as an apple, or even not larger than a walnut, contains many thousands of tubercles; it is quite a nest of them which enlarges, not by the growth of the original focus (granule), but rather by the continual formation and adjunction of new foci (granules) at its circumference. If we examine one of these perfectly yellowish white, dry, cheesy tubers, we find immediately surrounding it a soft, vascular layer which marks it off from the adjoining cerebral substance—a closely investing areola of connective tissue and vessels. In this layer lie the small, young granules, now in greater, now in less, number. They establish themselves externally [to the previously existing ones], and the large tuber grows by the continual apposition of new granules (tubercles), of which every one singly becomes cheesy; the whole mass, therefore, cannot in its entirety be regarded as a simple tubercle. The tubercles themselves remain really minute, or as we are wont to say, *miliary*. Even when on the pleura, by the side of quite small granules, large yellow plates, looking as if they were deposited upon the surface, are met with; these too are not simple tubercles, but masses composed of a large aggregate of originally separate granules.

"Here, you see, form and nature are in reality inseparably connected. The form is produced by the growth of the tubercle from single cells of connective tissue, by the degenerative proliferation of single groups of connective-tissue corpuscles. Thus, without more ado, it appears at once in the shape of a granule. As soon as it has once attained a certain size, as soon as the generations of new corpuscles which develop themselves out of the old histological elements by a continual succession of divisions, at last lie so close to one another, as to cause a mutual arrest of development, gradually to induce the disappearance of the vessels of the tubercle, and thereby to cut off their own supplies, then they begin to break up, they

die away, and nothing remains behind but *débris*—shrunk, disintegrated, cheesy material.

“The cheesy transformation is the regular termination of tubercle; but, on the one hand, it is not the necessary one, inasmuch as there are rare cases, in which tubercles, in consequence of their undergoing a complete fatty metamorphosis, become capable of reabsorption; and, on the other hand, the same cheesy metamorphosis befalls other kinds of cellular new-formations; for pus may become cheesy, and likewise cancer and sarcoma. This metamorphosis, therefore, being common to more than one formation, cannot well be set down as a criterion for the diagnosis of any particular structure, such as tubercle; on the contrary, there are certain stages in its retrograde metamorphosis, where one cannot help confessing that it is not always possible to come to a decision. If a lung be laid before you with cheesy masses scattered through it, and you are asked if that be tubercle or no, you will frequently be unable to say with certainty what the individual masses originally were. There are periods in the course of development when that which is inflammatory and that which is tuberculous can with precision be distinguished from one another; but, at last, there comes a time, when both products become confounded, and when, if one does not know how the whole<sup>a</sup> arose, no opinion can any longer be formed as to what its nature is. In the midst of cancerous masses also cheesy spots occur which look exactly like tubercles. I have demonstrated that it is by the gradual transformation of the elements of cancer that this cheesy matter is produced. But if we did not positively know from the history of their development that cancer-cells disintegrate step by step, and that no tubercles form in the middle of cancer, we should in many cases be altogether unable to arrive at any decision from merely examining the specimen.

“If these difficulties be surmounted which lie in the external appearance of the formation, and lead the observer astray not only when he considers its grosser features, but also when he investigates its more intimate composition, there remains nothing else to assist us in coming to a right conclusion than the investigation of the type of development displayed by the individual new-formations during the stages of their actual development, not during those of their retrograde metamorphosis. The nature of tubercle cannot be studied after the period when it becomes cheesy, for from that time its history is identical with the history of pus which is become cheesy; an earlier period must be chosen when it is really engaged in proliferation. So in the case of other formations, that period must be studied which is comprised between their origin and their culminating point, and we must see with what normal physiological types they agree. Then it is, I think, certainly possible for us to arrive at a just conclusion with the aid of the simple principles of histological classification, which I have already propounded to you. Heterologous tissues also have physiological types.”

In bringing his task to a conclusion, it is really surprising with what a species of fond regret a reviewer, more especially where he has found but little to awaken the bitterness of damaging criticism, finds himself compelled by circumstances to bid farewell to his author. The close attention which, if he is sincerely desirous of performing his task conscientiously, he must concentrate on the subject as necessarily creates a kind of consentaneity of idea between them, a feeling that gradually becomes so strong that we experience a certain natural reluctance in bursting too abruptly the intimate bond of connection that has thus been insensibly produced. The opinion which too universally prevails, and which we also fear is too generally believed, that men devoted to science are always too ready to sap the reputation of a rival in the same path of literature, whenever a favourable opportunity may present itself, is a gross and wicked libel, as utterly devoid of truth as it is indicative of the bad taste of those who can stoop to the propagation of a scandal so completely without any real foundation. The generous mind sees in the true follower and able delineator of purely scientific truths, simply the living impersonation of science itself; and its ardent votary bends the knee, without a single feeling of jealousy, at the same shrine with his fellow-worshipper—a community of feeling engendering a mutual respect, sufficient to annihilate altogether those contemptible exhibitions of petty rivalry which manifest themselves but too frequently in the other ordinary paths of life. The realms of knowledge, in their wide and unmeasurable expanse, present so many regions yet untouched, that there is abundant scope for all, without any danger of interference one with the other; and of this the work before us presents a striking example—it has opened a new era in pathological science, as depending upon microscopical revelations; and as such it must be carefully and universally studied by those who are anxious to maintain their proper ground in the estimation of the world, as being up to the standard of information of the present day. To the able translator, who has so faithfully accomplished the difficult task of transferring to a language not his own, not only the words, but likewise the exact sense and peculiar style of their author, our gratitude is especially due; for without his aid, this great monument of patient labour, accurate research, and stupendous learning, must have remained comparatively a dead letter, accessible to the few only, and not, as he has made it by his exertions, the clear and undisputed appanage of the many.

*Clinique Médicale sur les Maladies des Femmes.* Par M. G BERNUTZ, et M. E. GOUPIL. Paris: Chamerot, 1860. 8vo Vol. I., pp. 691.

THE work before us, though bearing the name of "*Medical Clinique on Diseases of Females*," is not intended, as Dr. Bernutz takes care to inform us in the very first lines of his preface, to treat of *all* diseases incident to females, but of those only "the existence and symptomatology of which is beyond all doubt." We have as yet received but the first volume of the work, and in it are treated only certain internal hemorrhages, viz., those depending on *retention of the menses, peri-uterine hematocele proper*, and on *intra-pelvic hemorrhage, depending on extra-uterine fœtation*. The principal interest of the work depends on the number of carefully collected and diligently studied cases of that form of hemorrhage to which the name of "peri-uterine hematocele" has been given—a form of disease which, though for several years both recognised and studied on the Continent, has till very lately been completely ignored by us at home. We quote the words of our able contributor, Dr. M'Clintock, who lately gave us an interesting account of a case of peri-uterine hematocele. He says:—"Dr. West and Dr. Tilt are the only English writers I know of who have written upon this disease, or met with examples of it; nevertheless, for the last ten years it has been the subject of many essays and memoirs by French physicians and surgeons, especially, MM. Huguier, Nelaton, Trousseau, Nonat, Laugier, Oulmont, Puech, Bernutz, &c., &c." This last is the author of the work at present before us, with whom is associated Dr. Goupil; but who, as we learn from the preface, only contributes the article on "uterine deviations." To the above list of authors who have treated of this disease, we can add the name of Becquerel, who wrote about it in his "*Traité Clinique des Maladies de l'Uterus, et de ses Annexes*," in 1859; and of Voisin, who, in 1860, wrote "*De l'Hématocele Rétro-Uterine*," &c., &c.

Dr. Bernutz divides into two well-marked classes all cases of menstrual retention, viz., such as depend on difficulty of *secretion*, and such as depend on difficulty of *excretion*;—he dwells on the great importance of carefully distinguishing the one from the other, as an error in the diagnosis would lead to still greater errors in the treatment.

After a rapid review of the theory of ovulation as the cause of the catamenial discharge, he recapitulates in a few words as follows:—"The menstrual excretion, in order to be effected

normally, requires a concurrence of several distinct processes; for instance, congestion, application of the Fallopian tube to the ovary, dilatation of the orifices, especially that of the neck of the womb; and if any of these should fail, either entirely or in part, such interruption of the function may result as may be productive of more or less serious complications, the cause of which must be fully recognised before an adequate and rational treatment can be applied." Peri-uterine hematocoele is probably not a very rare disease; and the author is inclined to think that the cause of its having been so long unobserved is due to the serious peritonitis which generally accompanies all effusions of blood into the peritoneal cavity; and which on the one hand monopolized the attention of the physician, whilst such hemorrhages as occurred externally to the peritoneum were erroneously considered as cases of metritis, or of phlegmons of the broad ligaments, and of the iliac fossæ. Having devoted only eight pages to these considerations, the author at once proceeds to the description of the several cases which form the body of the work, and which are full of interest; and we cannot do better than give a short epitome of some of the more interesting.

Case 1.—August 13, 1844. F—, aged 40, of delicate constitution, though she never had a positive illness. Menstruated at 17, and continued regular, until a short time since, except during her pregnancies, of which she had seven. During her first five pregnancies, she got herself bled; and on each occasion she had a premature confinement, between the sixth and seventh month. During her last two pregnancies she abstained from getting herself bled, and the confinement did not occur before its normal term. About four months since, after some hard work, during which she had to carry heavy loads a long distance, the menstrual period did not occur at the expected time; and in a few days the patient was seized with violent pains in the abdomen, similar to the pains of labour. After a few days, these pains diminished, and continued in a mitigated form till the next menstrual period, when a violent exacerbation occurred, but unaccompanied by any catamenial discharge. Together with the pain, she suffered much from nausea and vomiting, and had considerable fever also. The treatment consisted in leeches to the anus, blisters to both hypochondria, and a hot bath daily. About three weeks later, she was attacked by very violent pains at the time she was in the bath, which were followed by the passage of a membranous clot, followed by a slight and pallid discharge. After this occurrence, the patient slightly improved, until the

16th of August, when, having suffered another exacerbation, she sought for admission into hospital. Her condition was then as follows : she was emaciated and pale; the expression of her countenance was anxious; appetite, nil; no nausea, no vomiting; bowels, free; urine, normal. No pain in the lumbar region on pressure; abdomen clearly protuberant inferiorly and laterally into the iliac fossæ, extremely tender on pressure; no perceptible fluctuation; digital exploration *per vaginam* increased the pains excessively. The treatment consisted in leeching the abdomen, the use of baths and poultices, low diet (*diète*), and lemonade. She died on the 30th, two weeks after her admission. Her body was examined, forty hours after death, when the following condition of parts was discovered in the abdomen: the stomach, liver, intestines, &c., where in contact with the peritoneum, were found slightly adherent; and, in the depending portions of the cavity, there were about three pints of a brick-red liquid; the parietal peritoneum, of a slaty grey, was studded with red maculæ in its upper portion, whilst in its inferior half it became of a deep indigo, marbled with black.

The external surface of the intestines was similarly coloured, and the inferior six inches of the small intestine, together with the cœcum, were completely melanotic. At about the level of the superior strait, the following parts were found solidly matted together, forming a perfect division between the upper and lower regions of the peritoneal cavity, viz.: the uterus, bladder, two ovarian tumours, the sigmoid flexure of the colon, the inferior portion of the small intestine, the cœcum, and the vermiform appendix. The circumscribed cavity, of which the above-named viscera formed the roof, was lined by a much degenerated peritoneum, covered extensively with villous exudations of lymph; it was incompletely divided into two unequal cavities by an antero-posterior sort of membranous expansion, the right portion of which contained a quantity of brick-red liquid, while the left enclosed a large and firm clot, having the shape of a truncated cone. The tumours, which we have perhaps improperly called ovarian, were of unequal size; the one to the right, not larger than an egg, of a deep red colour, and studded with villousities, was found, on examination, to be formed by the fringed extremity of the right fallopian tube, which so firmly adhered to its corresponding ovary, that, in attempting to separate the two, the substance of the tube itself gave way before it could be separated from its adhesions; the cavity of the tumour was filled with altered blood, pus, and clots, while the intra-uterine portion of the

tube was full of crimson blood. The tumour on the left side was considerably larger, and differently constituted—its boundaries being formed not only by the fringed extremity of the Fallopian tube and its corresponding ovary, but also, in great part, by a portion of parietal peritoneum, together with adventitious membranes connecting the two. The uterus was simply hypertrophied, especially at its neck, and its cavity dilated. No communication could be detected between the cavities of the tumours and that of the pelvi-peritoneal cyst.

The author concludes from the above examination as follows:—

“The hypertrophy of the neck of the uterus, acting in this case similarly to an enlarged prostate in man, interfered with the free exit of the discharge at each menstrual period, producing a high degree of congestion of the generative organs in the first instance; distention of the cavities, and formation of the hypogastric tumours, in the second; whilst at the third and fourth periods, there were further distention and laceration of some of the false membranes, by means of which the blood found its way into the cavity of the peritoneum, where it excited more than one attack of inflammation, which eventually caused the death of the patient.”

The study of the foregoing case induced the author to reject as *independent idiopathic affections* the following diseases viz., *uterine congestions: metrites; phlegmons of the broad ligaments; inflammations of the ovaries and the peritoneum, fleshy moles; and dropsies, either of the uterus, Fallopian tubes, or ovaries;* and to refer them all to one common source, viz., retention of the menstrual flux.

We have no doubt that many of these diseases may arise, and do arise, from retained menstrual flux; but we cannot agree with the author that *all* do; for instance, *fleshy moles* (*môles charnues*), are the result of some irregular, vitiated, or arrested form of conception, as is evident from their partial organization, and the heterogeneous substances—hairs, bones, teeth, &c., &c., found generally in them. Had the author spoken of “*Môles Hydatiques*,” instead of “*Môles Charnues*,” there he would have been on more debateable ground, as opinions are considerably divided upon that subject. Ruysch, Sir C. M. Clarke, and Evory Kennedy believe that they may occur in virgins; while Denman, Capuron, Boivin, and the late Dr. Montgomery, consider them to be always the result of impregnation.

Having referred all the above diseases to retained menstrual flux, the author proceeds to enumerate the different



causes which may produce that retention; and classes them under eight heads:—

1. Imperforation of the vulvo-uterine passage, congenital or accidental, previous to puberty, occasioned by cicatrices, &c., &c.

2. Obliteration as above, but posteriorly to puberty.

3. Partial obliteration of the vagina, or the neck of the womb, either congenital or accidental.

4. Hypertrophy of the neck of the womb.

5. Accidental occlusion of the neck of the womb by polipi, adventitious membranes, &c., &c.

6. Deviations and flexions of the womb, whatever their origin.

7. Spasmodic contraction of the neck of the womb.

8. Abnormal functional condition of the Fallopian tube, either congenital or acquired.

We have given this classification *in extenso*, out of regard to the author,—not, however, that we approve of it; for many of the distinctions are, in fact, without a difference, and serve only the double purpose of increasing the volume, and mystifying the reader. Not content, however, with this arrangement, he further subdivides the first class into four sub-classes; but we cannot follow him in these minutiae, and prefer passing on to some of the interesting cases he records—all full of instruction, and many painfully so, from the reckless mode of treatment adopted.

Case 2. A. B., aged 22, never had menstruated; since the age of 13, had suffered every month, for three or four days, paroxysms of pain, which returned with unfailing regularity. For some years back, the abdomen has been considerably enlarged; and, although her general health continued good, yet the monthly attacks of pain continued to recur. A vaginal examination revealed the presence of a membrane completely stretched across the vagina, behind which it was clear that fluid was existing. This membrane was crucially incised; a quantity of blood came away, as also some pints of a black inodorous fluid, without clots. For three days the menstrual discharge continued, without producing any unpleasant symptoms; the abdomen diminished in size, and the patient appeared to be recovering; when, on the fifth day, she was attacked by peritonitis, and died nine days after the operation.

*Sectio cadaveris*.—Peritoneum inflamed, intestines covered with lymph, and bathed in sero-purulent fluid, uterus larger than usual, os tincæ patent, vagina enormously distended; the uterine half of the Fallopian tubes was enlarged to the size of

a man's little finger, and distended with black blood; the ovarian extremity of the tubes was so attenuated, that the slightest pressure would cause the blood to ooze out.

Case 3 is similar in its history to the previous one. Relief was given by puncturing the imperforate hymen, which was the cause of all the mischief; everything went on well till the fourth day, when she was seized with peritonitis, and died on the fifth. At the post-mortem examination, in addition to the usual appearances of peritonitis, there was found a quantity of a tarry liquid, similar to what had escaped from the vaginal puncture during life; the Fallopian tubes and ovaries were enlarged to such a degree as to form cysts capable of holding a pint each, and in their walls were ulcerations, through which the tarry fluid had found its way into the cavity of the peritoneum.

Case 5 is taken from the London Medical Gazette, and related there by Sir B. Brodie; it was pretty similar to the preceding one; was treated by puncture of the occluding membrane. Death occurred a few days after from peritonitis. On examination of the body after death, a large quantity of menstrual blood was found in the peritoneal cavity; but no laceration whatever, either in the uterus or the tubes, by which the blood could have escaped.

Case 6. E. D., aged 26, had enjoyed excellent health up to the twentieth year of her age, when she was attacked by violent abdominal pains, which recurred monthly.

After some time, these recurring pains having become intolerable, and the patient having lost her previous good health, she consented to a local examination, when it was found that the vagina was closed at about its upper-third, by a transverse membrane. It was incised with a narrow knife, carried forward on the index of the left hand, when a quantity of dark grumous blood was evacuated. The abdomen, which previously had been tumid; became flat; and the patient did not complain of uneasiness until after a few hours, when she was seized with convulsions; the abdomen again became tense; she was attacked by peritonitis, and in two days was dead. At the post-mortem examination, sero-purulent fluid, and putrefied blood, were found in the cavity of the peritoneum; the tubes were closely adherent to the ovaries, and greatly dilated; one of them presented a considerable *rent*, through which a semi-putrefied blood was freely passing into the peritoneal cavity.

Case 7. Similar to the above, only that death occurred naturally; no operation having been performed.

Case 8. Similar to Case 6; operation, and death.

Case 14 is interesting, because we cannot help thinking that, if some treatment had been adopted, at the same time that the patient had been supported by food and stimulants, her life might have been saved, as she appears to have sunk from exhaustion.

Case 22. Menstrual retention, caused by cancerous growths of the neck of the womb; after some time, the womb increased considerably in size, when, after five days of violent pains, like labour pains, a large clot was expelled, the uterus diminished, and the pains ceased.

Case 23. Menstrual retention, due to the presence of a polypus; intense pains at each recurring period; removal of the polypus; recovery.

Case 27. Taken from the "London Medical Gazette," of 1846, is related by Oldham, and considered by him as a case of *pseudo-membranous dysmenorrhœa*. The author does not take the same view as Oldham, and does not believe in the *ovarian influence* to which Oldham says the disease is due. He entirely agrees with Tilt, whose words he quotes from the "Lancet," of 1853, and further adds:—"The existence of the lesions indicated by Mr. Tilt, in the preceding autopsies, completely annihilate all Mr. Oldham's arguments, which are solely based on the microscopic examination of the dysmenorrhœal membrane."

Case 28 is interesting from its cause principally. Julia Dufour, of dark complexion and high colour, 37 years old, was always regular since the age of 14, when she first menstruated; has no children; is not happy with her husband. Being of a violent temper, she often gives way to fits of anger, which have often been followed by excessive menorrhagia. On one occasion, however, her fit of rage coming on during a menstrual period, it caused its suppression, which was followed by very serious complications, including the appearance of a considerable hypogastric tumour. She was treated with ergot; and, after a time, having expelled a quantity of clotted semi-putrefied blood, she recovered.

Case 31 is of considerable interest, as, during life, it was supposed to have been one of peri-uterine hematocèle. A. W., aged 28, fair and slight, has never menstruated; up to the age of 24, she never even suffered from any excitement of the genital organs. She has a sister who menstruates regularly, but always with excessive pain. At the age of 24, A. W. was attacked by violent pains in the back and in the lower belly, which, after a few days, ceased, but returned again a month later; and continued thus returning month after month, with

unabated violence, for the space of four years. At the end of this time she came into hospital, when her condition was noted as follows:—abdomen tumid, resonant under percussion in its upper half; absolutely dull inferiorly. By careful palpation, a large, rounded mass can be felt in the right iliac fossa. The index finger of the right hand being introduced into the vagina, it meets with the lower surface of a tumour, about the size of the head of a fœtus, apparently impacted in the upper strait. This tumour is hard, and painful on pressure; nowhere can fluctuation be detected, neither can any arterial throb be felt over its surface. After a few days, a slight discharge made its appearance, accompanied by a few clots of blood, at the same time that she was seized by fever, accompanied by violent delirium, which, however, subsided on the application of twenty leeches to the abdomen. She had now been a month in hospital; and her sufferings being on the increase, M. Huguier, under whose care she was, determined upon performing an operation for her relief; this was to consist, first, of a puncture into the tumour through the lower part of the abdomen, which was then to be continued, by means of a long, curved trocar, through the inferior portion of the tumour, into the vagina. The first portion of the operation being completed, exit was given to a quantity of thick, chocolate-brown liquid, full of minute clots of blood. Before, however, proceeding to the second step of the operation, M. H. thought well of introducing a canula into the bladder, when, by exploring both by the vagina and the rectum everywhere, the finger encountered the canula in front of the tumour, as if, in fact, the rectum and the bladder were spread out over it, and consequently the second step was not proceeded with. Immediately after the operation, the patient got a prolonged rigor, and afterwards complained of agonizing pains. She was treated with mercurial inunctions, leeches, and croton oil frictions on the thighs.

Five days later, she being somewhat recovered, and the tumour having again filled up, M. Huguier made a further examination, both by the vagina and by the rectum, when, having ascertained that for a space of about two centimetres, the tumour was uncovered by either rectum or bladder, he determined to puncture it through that space, and gave exit to a large quantity of extremely fetid greyish-coloured sanies, accompanied by puffs of fetid gases; a gum-elastic tube was introduced, and a solution of iodine injected. The patient was instantly seized with vomiting, violent pains, facies Hippocratica, and matters appeared to be drawing to a conclusion. 20

leeches to the abdomen, and some tincture of aconite, relieved these symptoms, and on the following day the patient was better. Thus she continued, one day improving, and retrograding the next, for sixteen days, when she was attacked by phlebitis, and died four days later. The body being examined, the intestines were found glued together by lymph, and bathed in dark fetid pus. The liver was immensely enlarged, while the rectum, uterus, broad ligaments, and bladder, were covered with a thick layer of pus and false membranes. The left Fallopian tube was found dilated in its entire length; the fringed extremity closed, forming with the tube a sort of club-shaped tumour, of the size of a pigeon's egg. On cutting into it, some chocolate-brown fluid made its escape. The left ovary was enlarged and softened. On the right side, all the pelvic cavity was filled by an irregular anfractuous sac, the inferior portion of which was gangrenous, and of a dark-grey colour; at the most depending part could be seen an ulcerated opening (formed by the trocar), which, passing between the rectum and the bladder, communicated with the vagina. The uterus healthy, though adherent by strong false membranes to the bladder. The right ovary and right Fallopian tube could not be recognized.

The retention of the menstrual discharge often produces, as we have seen, very serious accidents; and in the long run, may give rise to such serious complications as to imperil the life of the patient. We have seen that at first the symptoms are slight, being chiefly pain in the lower belly, accompanied by a sense of weight in the pelvic basin. At the next menstrual period, however, these pains are of a more decided character; and at the next period again, some additional symptoms generally are superadded, viz., pains in the lumbar and sacral regions, with pressure, and bearing-down, simulating in a great degree the pains of labour. If no relief is given by affording exit to the pent-up secretion, either by the unaided efforts of nature, or through surgical assistance, the disease will further progress; and at future menstrual periods, feverishness will be superadded to the previous symptoms; tumour on tumour will probably arise in the iliac fossæ; and if still no external exit is provided, the menstrual fluid, having dilated the tubes and the fringed extremities beyond their power of resistance, will find its way into the abdomen, and thus give rise to peri-uterine hæmatocele, of the encysted variety, should the flow of blood into the peritoneal cavity be slight and gradual; and of the unencysted variety, should it be sudden and abundant.

Should a case present itself where the catamenia have evi-

dently been trying to establish themselves at several periods, but where, owing to an occlusion of the vagina, it is physically impossible for them to find an exit, is it prudent for the surgeon to interfere, and how? We cannot fail to be struck at the number of deaths recorded in this work after surgical operations; and *that*, even when apparently the most trivial puncture was resorted to only. Dr. Bernutz is of opinion that when surgical interference is determined on, it must not be adopted too late. He thinks that hemorrhage into the peritoneal cavity has often been *the consequence of the operation itself*, from its having startled the uterus into more vigorous contraction, at a time when both it and its tubes were gorged with blood. We would suppose also that the nature of the occlusion would have the greatest influence upon the likelihood of success in operating. For instance, Amussat relates an interesting case of occlusion of the vulva from close apposition of the posterior surface of the bladder to the rectum, in which he succeeded in forming an artificial vagina, by pushing a way, with his finger between the two viscera, and thus by degrees reaching the neck of the uterus. The case is detailed at length in the work before us, and deserves a careful perusal. In juxtaposition to this successful interference is a fatal one, related by de Haen, in which the bladder was opened into, in an attempt to reach the womb through an imperforate vagina. Now, there is considerable difference of opinion about the proper time for adopting surgical interference. Nélaton, one of the most able surgeons of the present day, and who has devoted much time to the study of this subject, invariably delays as long as possible; and only proceeds to open these tumours when either the pains they cause are no longer tolerable, or when he fears that, by ulcerating an exit for themselves through the rectum, a still greater danger may be incurred by fecal matters entering the cyst.

Another question which presents itself for consideration, is the *proper time* for the surgical interference to be put into practice. Some authors recommend that a menstrual period should be the time selected; but Dr. Bernutz is of the opposite opinion, and believes that there is much less danger in perforating the hematocele, through the vagina, during a period of quiescence, viz. about 8 or 10 days after a menstrual period; and especially cautions the operator not to attempt to press on the abdomen, by way of aiding the evacuation of the fluid. He also advises that no large opening of either the hymen or other structure should be made with a bistoury, but that a small trocar should be selected in preference, and at the same time

to avoid a too rapid evacuation of the fluid. He also advises not to leave a canula in the opening, as has been recommended by some; for he thinks that the irritation caused by the presence of a foreign body would be more prejudicial than any additional puncture which might have to be made on the previous opening becoming healed.

Having exhausted the subject of menstrual retention and its consequences, the author now enters upon the subject of *Peri-uterine hematocoele and its varieties*. We were under the impression that for a long time we had been considering this disease, but it seems we had misunderstood the writer. Let us hear him speak for himself:—

“In the preceding memoir I have especially insisted upon the severest accident which may arise, under certain circumstances, in consequence of the menstrual discharge being retained, viz., upon the passage into the peritoneal cavity of the blood previously contained in the genital organs, and which would give rise to a more or less violent peritonitis, and to the production of an intra-pelvic tumour, which, since my first publication upon this subject in 1848, has received the name of *retro-uterine hematocoele*. The date of this publication, together with all the time and labour I have devoted to establish the relation between intra-peritoneal hemorrhage and disturbed catamenial function, would justify me in considering myself, rather than any of my contemporaries, the discoverer of this form of hematocoele. But I do not lay claim to it, because, as I already declared in 1848, this discovery is really due to Ruysch. But I do claim for myself the merit of having been the first to bring forward this discovery, which, up to this time had remained as a simple indication, and to have established the basis from which so many recent works upon hematocoele have sprung—and that, notwithstanding the studied silence of M. Nelaton, who, up to the present time, has avoided making the slightest mention of my memoir, although he has largely drawn from it, for his lectures on hematocoele.”

The great Parisian surgeon evidently finds no favour in the author's eye, and further on we again find him at issue with our friend Nelaton.

“I will not, like M. Nelaton and his pupils, gather into one group extra-uterine conceptions with congenital imperforations, menstrual retentions, all sorts of hemorrhages—rupture of aneurisms and tubo-ovarian varices, with hemorrhagic peritonitis, and thrombus, and to this incongruous collection give the name of disease,—but I shall rather endeavour to discriminate one from the other these different varieties of bloody

effusions, the pathology of which it is most important to recognise."

Having in a few words shown the physiological analogy between the testicles in man, and the ovaries in woman, the author insists that the name hematocele should be retained solely in those cases where the blood is poured into the peritoneal cavity; whereas the extra-peritoneal effusion, resembling rather bloody infiltrations of the scrotum in man, should never be distinguished by that name, no more so than when they occur in the areolar tissue of the scrotum. Having established the real significance of the name, he eliminates from the consideration of the present work all *extra-peritoneal* effusions of blood, as also the intra-peritoneal ones, which depend on abdominal injuries, rupture of aneurismal sacs, perforating ulcers, and accidents from extra-uterine conceptions, and reserves the name of hematocele for those collections of blood only, which are analogous to effusions of blood into the tunica vaginalis. Genuine peri-uterine hematocele the author divides into three classes,—such as arise from laceration of either the ovary or the Fallopian tube—such as depend on impeded excretion of the menses;—and, lastly, such as arise from a general hyperemic condition of all or some of the genital organs, and in which, although there may be no difficulty of excretion, yet there is at the same time an oozing of blood into the peritoneal cavity, through the open ends of the Fallopian tubes. Of the first class are three cases, which the author details at length, and in which death occurred in two instances. The cadaveric section of these two showed the origin of the hemorrhage to have been a varicose condition of the "*plexus pampiniforme*." In all these cases the *immediate* cause of the hemorrhage was severe bodily exertion. How can a case of hematocele depending on varicose condition of the ovarian veins be diagnosed during life? Dr. Bernutz believes that women who have borne many children, and in a comparatively short period of time, are more subject to this form of hematocele; and that, if suffering from varices of the extremities, and, still more so if they have a varicose state of the veins in the vicinity of their genital organs, the physician may be guided to infer from these facts that the case is one depending on varix of the *plexus pampiniforme*. The treatment he recommends consists in cold applications to the abdomen, leeches to the neck of the uterus, and absolute rest; and, strange to say, he not only condemns "une diète trop severe," but on the contrary recommends a certain amount of nourishment, as an anemic condition might increase the hemorrhagic tendency. We said, strange to say,



because, in French medical practice, "starvation" holds such a prominent place, and the phantom "inflammation" is so constantly haunting their imagination, that one is agreeably surprised to see them leave their "diète absolue," their "sangues," and their shocking "tisanes," and adopt at least a more rational and physiological empiricism. Should the patient recover, then she should carefully avoid for the future all excessive fatigue, and especially keep at rest at the approach and during the continuance of a menstrual period.

The author speaks of excessive venereal excitement as a cause of hematocele; and quotes two cases related by Tardieu, in which death supervened so closely upon an orgie, as to give rise to suspicion of poisoning, but at the post-mortem the cause of death was patent; an accumulation of blood was found in the peritoneal sac, without any opening or laceration whatever either in the ovaries or the tubes. Where did the blood come from, then? The author supposes that, as in cases of direct violence, effusions of serum and blood take place in the tunica vaginalis of man, so in woman the same thing may occur without the necessity of a previous solution of continuity in the corresponding parts. Another cause of hematocele is rupture of the ovary; and at observation 7, quoted from Drecq, an interesting account is given of a case in which death supervened in thirty hours from this cause, and in which the lacerated ovary was found of great size, and more like the spleen of an individual dead from scurvy.

Observation 9 contains the details of another interesting case, in which, after the suppression of the menses in consequence of a mental shock, severe peritonitis set in; death occurred in three days, which was at the examination of the body found to depend upon the laceration of one of the ovaries. We could extract many other cases of interest; but we must pass on to the third and last memoir of the volume before us, which treats of intra-pelvic hemorrhages depending on extra-uterine pregnancies.

"Having given, in the preceding memoirs," says the author, "a general account of intra-pelvic hemorrhage arising from the organs of generation while in a state of vacuity—we conceived that the subject would be incompletely dealt with, if we did not devote some space also to those intra-pelvic hemorrhages which occur during pregnancy, whether intra or extra-uterine. We shall, however, only occupy ourselves with the latter, not only because we consider the accidents depending upon the former to belong, strictly speaking, to the domain of obstetric science, but principally because these hemorrhages,

with very few exceptions, depend either upon apoplexy of the ovaries, or rupture of the Fallopian tubes or of the venous plexusses, and belong to the pathology of thrombus, already so ably treated by Deneux (*Recherches pratiques sur les tumeurs sanguines de la vulve et du vagin*, 1835), that we could add nothing to it; and our description would only be a paraphrase of his original work, to which we refer our readers."

Although extra-uterine conceptions are rare, yet they are sufficiently frequent to have enabled the author to collect forty-two cases, from the study of which he has deduced the following conclusions.

When hemorrhage occurs as a consequence of extra-uterine pregnancy, death almost always follows so quickly on the accident, that rarely is there sufficient time for the formation of an encysted hematocoele.

The origin of the hemorrhage being various, he takes it as the basis of his classification, and divides all hematocoeles, arising from this cause, into five orders :—

1. Caused by the rupture of the dilated utero-ovarian veins.
2. Caused by rupture of the ovary.
3. Caused by rupture of the Fallopian tube.
4. Caused by rupture of the foetal cyst itself.
5. Caused by hemorrhage into the foetal cyst, without any solution of continuity of its walls, and which, without any flow of blood into the peritoneal sac, may rapidly induce the death of the patient.

Now, considering that this portion of the work before us is devoted to the consideration of those sanguineous effusions, which depend *especially on extra-uterine pregnancies*, we were not prepared to find classed under the first, second, and third divisions, cases of hemorrhage, which might just as easily occur during an ordinary pregnancy, or without any pregnancy at all; we would, therefore, really be justified in skipping over all the author has to say upon the subject of his first three classes, and pass on at once to the review of the fourth and fifth; but having followed him step by step through his interesting, although prolix and wordy, publication, we shall follow him to the end, even through those cases which have no bearing on the subject in hand.

Observation first, taken from the writings of Ollivier D'Angers, is a case of laceration of the utero-ovarian veins, through which four pints of blood were poured into the sac of the peritoneum, causing death within seven hours from the occurrence of the first symptom. Just above the dilated plexus of veins

from which the hemorrhage had taken place, the Fallopian tube was dilated for the space of about three inches in one direction, and one inch and a half in the other; and upon this being opened, a foetus of about five weeks was discovered; thus proving this case to have been one of extra-uterine conception. But we cannot at all see the necessity of making the varicose condition of the ovarian veins, and the subsequent rupture of them, to *depend* upon the abnormal pregnancy, as both might have existed independently of each other.

Case 2 is also one of extra-uterine conception, in which death occurred five days after the appearance of the first symptoms. The real nature of the case was diagnosed by M. Nonat during life. But at the cadaveric section was found what had never been anticipated, viz., pregnancy in the Fallopian tube *of one side*; whilst the origin of the hemorrhage was *ovarian apoplexy of the opposite side*. So this case also, equally with the preceding one, could have occurred quite independently of any conception at all.

Case 3, extracted from the "*Mémoires de l'Académie des Sciences, 1702*," is one of extra-uterine pregnancy, in which death occurred at about the sixth week, three days after the patient falling upon her knees. At the post-mortem examination, the left Fallopian tube was found torn, and in the rent, a vesicle containing a foetus.

Case 4 is similar to the preceding one, with this peculiarity, that it would appear there was at the same time both extra-uterine and intra-uterine pregnancy.

Case 12 is also one of extra-uterine conception, with intra-pelvic hemorrhage, which last had been recognised during life. But the pregnancy not being suspected, and in the idea that it was a case of peri-uterine hematocoele, a puncture was made into the tumour through the vagina; and after letting out some blood, but no clots, an injection of tepid water was thrown up through the incision, when immediately the patient complained of agonizing pain; she became deadly pale; rigors, followed by unceasing vomiting, set in; and in ten hours she was dead.

Space forbids us adding any more to this already lengthy analysis of a work which, even though tediously written, still deserves the attention of those who make diseases of females their special study; and to whom we recommend it, with the full conviction that they will feel interested, at the same time that they will gain information in reading the many carefully-related cases which form the bulk of the work.

*On Diphtheria.* By EDWARD HEADLAM GREENHOW, M. D., &c. London: Parker and Son. 1860. 8vo, pp. 274.

DR. GREENHOW seems to have been stimulated to write this excellent treatise, by his appointment under the Privy Council, in the year 1859, for the purpose of making an inquiry into the causes, symptoms, and treatment of diphtheria. The report of the results of that inquiry has already been published, at any rate *printed*, in the second report of the medical officer (Mr. Simon) of the Privy Council. In the treatise before us, the author incorporates his private experience and research with the products of his public labours, as a sanitary investigator, on the largest scale.

In the first chapter, we are presented with a "definition of the disease." In performing this task, the author falls into a description of its general phenomena, and does not attempt a strict definition. The truth is, that, supposing the author's view to be correct,—which presupposes the unity of epidemic sore throat without membranous exudation, and of true membranous angina attended with pellicular deposit,—the only basis of a completely satisfactory definition, such as has been put forth by Bretonneau and the French school, is at once cut from under us. We assent, in the main, to the author's doctrine, though it is most inconvenient in a nosological point of view.

It must not be forgotten, however, that we cannot pronounce rigorously a given throat to be a case of diphtheria, unless some part of the faucial or buccal mucous membrane displays a whitish, whitish-yellow, or buff-coloured membraniform layer, more or less extended, frequently stratiform, and for the most part easily removed, leaving the subjacent parts red, with or without minute red puncta, either perfectly whole, or partially broken up. The author's description of this exudation is true to nature, both in its positive and negative aspects; for we are all aware how much nonsense has been written upon this subject.

"Hemorrhage from the nose and throat, independently of the co-existence of purpura, often occurs in the course of diphtheria, and is sometimes very profuse."—p. 8. It is remarkable how constantly the earlier authors of the 16th, 17th, and 18th centuries allude to this symptom, and always as being one of very sad augury. We have seen fatal cases from hemorrhage during diphtheria, and strongly urged the physician to give a guarded prognosis, even when a moderate flow of blood comes from the

nares or fauces. It is agreeable to remember that the modern methods of treatment have disarmed this evil of half its dangers.

In the second chapter, diphtheria is described as it occurred in the 16th, 17th, and 18th centuries. The evidence, that the disease which ravaged various parts of Europe in these centuries—Spain in the 16th, Italy in the 17th, and our own country in the middle of the 18th century—is identical with that which has attracted the attention of the public in our times, is marshalled in lucid order, and produces the impression on the mind that further discussion on this point is superfluous. In fact, we may fairly challenge the most learned critic to state another disease, the description of which by various authors, far removed in place and time, presents so much similarity. In reference to Britain, the name of Fothergill, whose “account of the sore throat attended with ulcers”, was published in 1748, deserves honourable mention. His essay is a masterpiece of learning and of acute observation, but has contributed much to the confusion of some authors, owing to his having described cases of scarlet fever with membranous sore throat, as examples of his gangrenous angina. The opposite error was made by the accomplished Withering, who founded sore throat with scarlet fever, as shown by the very title of his book, “An Account of the Scarlet Fever and Sore Throat or Scarlet Fever Anginosa; particularly as it appeared at Birmingham, in 1778.” For our own part, we must express our gratitude to Fothergill, for having given us a ray of light in the darkness which enshrouded the beginning of this epidemic. When, in 1856, we first saw cases, and uniformly fatal cases too, of this cruel malady, we sought for help among the authorities of the profession. The delineations of our class books were found uniformly untrue to nature; and the treatment advised,—based, for the most part, on purely speculative grounds, or on the fancied *identity* of the epidemic diphtheria with sporadic croup, or, worse than all, founded on second-rate French imitations—we bitterly discovered to be something worse than nugatory. In this strait, how dear to us became the life-like outline of Fothergill! How clear and strong the common sense of the old quaker of the Second George’s time! It affected our mind like a new discovery; and we loudly exclaimed, in lecture and in private, against the retrogression of British medicine\*.

The third chapter is devoted to the description of “diph-

\* It may be well to mention that Fothergill’s epidemic is the same as that described by Huxham, at Plymouth, the latter occurring two or three years later.

theria in the 19th century;" and we need scarcely state that the illustrious name of Bretonneau takes the foremost place here. His clear account of the local characters of the disease, the learned criticism with which his works abound of the labours of his predecessors—above all, the excellent characteristic term (*diphthérite*) which he introduced into medical literature—give him an importance in the history of the disease, which time will not obliterate. His reputation has, indeed, unfairly overshadowed that of all previous writers. No candid reader can peruse the essays of many predecessors of Bretonneau without seeing that in acute clinical observation their superiority is manifest. His title to our respect is mainly founded on his thorough recognition of the *whitish layer being not a slough, or anything of the nature of a slough, but a bond fide layer of exudation on the surface of the affected parts*. He believed this membraniform layer characterised the disease, hence he called it *diphthérite*. He pursued this view throughout every possible line of injury.

"To him (as Dr. Greenhow observes), no inflammation unattended by exudation is *diphthérite*; and no inflammation attended by exudation is *diphthérite*, unless it be propagable by contagion; the virus being the membranous exudation which forms the distinctive pathological feature of this disease."

Further, the physician of Tours teaches that a true inoculation is the only mode of transmission of the disease. These doctrines are seductive in their simplicity, and form the French gospel of diphtheria, but we do not doubt that they are too absolutely laid down.

Bretonneau bestowed the greatest attention on the croup-phenomena of diphtheria; but, in every essential particular, his observations had been preceded by those of Dr. Mackenzie and Dr. Brown, of Glasgow:—

"In a brief paper, published in 1825, Dr. Mackenzie says that the exudation of fibrin in croup 'very frequently commences on the surface of the tonsils, thence spreads along the arches of the palate, coats the posterior surface of the velum palatæ, sometimes surrounds and encloses the uvula; and at last, descending, covers the internal surface of the pharynx and œsophagus, the larynx and trachea.'"

In May, 1820, Dr. Brown read a paper on the disease to the Glasgow Medical Society.

In 1821, two cases of the disease proved fatal, under Dr. Mackenzie's care:—

"In both, the fetor of the breath, and the sloughy appearance of the effused lymph, were remarkable. In the first case, I was surprised, on dissection, to find the tonsils and uvula entire, and coated over only with an effusion; for I had laid my account to find a gangrenous loss of substance in these parts. He now announced to several of his medical brethren, that what had been considered as ulcers and sloughs in this disease, were nothing else than effused lymph, the progress of which over the velum and uvula and towards the alimentary and respiratory passages, he had distinctly observed".

It is remarkable how little interest was excited in Great Britain by these accounts, until the outbreak of the great epidemic still figuring in our bills of mortality. Boulogne was the scene of a frightful mortality from diphtheria, from early in 1855 to 1857, though the boarding-house keepers of that town stoutly denied it in the English papers. It also prevailed in various parts of France, in 1856; and it is worthy of note, that this French epidemic differed from those described by Bretonneau in the very important particular, that the disorder more rarely extended to the larynx, causing croup symptoms. This peculiarity has existed throughout the English epidemic, which began in the summer of 1856, in the north and central-midland districts. Dr. Greenhow justly remarks, that dropping cases, isolated specimens of the disorder, had occurred in various parts of the country for years previously, but they had attracted no notice. Soon the disease spread over nearly the whole country, but by no means with the rapidity which is so terrible a feature of cholera and influenza. Here, too, we observe a point of similarity with the disease, as delineated by the older authors.

Dr. Greenhow makes some interesting observations on the existence of a "diphtheritic taint" in other maladies during the prevalence of the epidemic; and quotes several good illustrations of this feature of diphtheria.

In the fourth chapter, the author has brought together, from numerous sources, proofs of the existence of "sporadic and endemic diphtheria" during the last thirty years, and passes on in the following chapter to the discussion of the non-identity of "diphtheria and scarlet fever."

The author candidly admits that he was at first inclined to believe that diphtheria is but a modified form of scarlatina. He

\* Quoted by Dr. Greenhow, from the *Medico-Chirurgical Review*, N. S., vol. vi. p. 290.

is now, however, satisfied that, "notwithstanding their frequent concurrence in the same place, and their occasional coincidence in the same individual, diphtheria and scarlet fever are distinct diseases," p. 105. This opinion we have ever strongly held, and can only assent to the fact of a closer affinity between diphtheria and scarlatina than between the former and any other specific febrile disorder. The existence of albumen in the urine in many cases of diphtheria, so well pointed out by Dr. Wade, of Birmingham, has been much relied upon in proof of the identity of these disorders; but it must not be forgotten that the presence of this principle in the urine is common to many of the pyrexial disorders, especially to the exanthemata; and moreover that, as contrasted with scarlatina, there are points of difference of a striking kind. In diphtheria the albumen appears early, often disappears in the course of a day or two, and is almost never complicated with anasarca. The opposite to this holds good in scarlatina. Dr. Greenhow insists strongly upon the fact of repetition of diphtheria attacks, as differentiating the malady from scarlatina, and refers to illustrative cases. Judging from our own experience, we should consider a second attack of diphtheria to be almost as rare as in the exanthemata. An unequivocal example of a new attack in the same individual has not yet come under our notice.

The following chapter alludes to human and brute diseases coincident with diphtheria. Among the former are enumerated puerperal and typhoid fever, erysipelas, carbuncles, and boils. Among the latter are the "eruptive disease," or "the foot and tongue disease," which appeared among the cattle of this country about the year 1839; and the murrain, or "lung disease," which broke out a year or two later than the preceding. It is a noteworthy fact, that these same brute disorders prevailed also just before the outbreak of diphtheria in the middle of the last century. The whole chapter is very interesting.

After adducing a series of facts and arguments, showing that putrid effluvia, locality, &c., exercise no recognisable influence on the generation of the disease, Dr. Greenhow discusses the "communicability of diphtheria," and supports his view of its contagious nature by the detail of cases. But it is merely stated that the disease is "in some way or other communicated" — Bretonneau's doctrine of inoculation being thought to be doubtful. The facts reported to us from France, from time to time, seem to prove that the disease may be communicated by this method. Not to allude to the well-known cases of Valleix, &c., another illustration has just occurred,



while we write, in the person of Dr. Gendron, who obtained his fatal attack, owing to a quantity of the secretions of the mouth being suddenly expectorated on his face during the performance of tracheotomy. We strongly concur with Bretonneau's doctrine, so far as relates to inoculation being *one* of the modes of transmitting the disease; but we cannot believe this to be the only mode of conveying it. We would here draw attention to the fact, that diphtheria is far less contagious than scarlatina or variola; and we believe its *contagious sphere* to be more circumscribed than that of these maladies. In a large house, children will be often safe if they are completely secluded from the sick chamber.

The following chapters are occupied with a careful and complete account of the symptoms, grades, and varieties of the disease, and of its morbid anatomy:—

“Low forms,” says the author, “of cryptogamic plants are occasionally found on the exudation, a circumstance which gave rise to the belief, that the disease is of parasitic origin. This opinion is disproved by the facts that, on the one hand, the supposed parasite is not invariably present in diphtheria; and, on the other, that it is frequently found on unhealthy mucous surfaces, which are not of a diphtheritic nature. Examined under the microscope, the exudation is found to consist of coagulated fibrine and epithelium; the latter being usually more abundant in the outer portion or layer of membrane, whilst the deeper portion is more purely fibrinous. But, in this respect, there are numerous variations. Exudation-cells are often intermixed with the fibrillated texture.”

The last chapter is devoted to the treatment. Dr. Greenhow is adverse to the rude local treatment, now so much in vogue, and condemns the stronger applications, excepting in the earlier stages of the disease. We are disposed to agree with much that he offers on this point, being well assured that these measures have been used too freely, and, above all, *for too long a period*. But we would not willingly dispense with the *moderate and early* employment of the dilute hydrochloric acid, or of the same acid a little weaker than the dilute acid of the London Pharmacopœia. The employment of strong solution of nitrate of silver should be limited to cases where either the exudation has not yet appeared, or has been peeled off mechanically, or dissolved by the acid application.

As regards the general management, we can earnestly recommend the perusal of the author's cases, as well as of his remarks, which, in the main, agree with our own experience. The remedy which he has chiefly relied upon, judging from the detail of the cases in the eighth and ninth chapters, is the

tincture of the sesqui-chloride of iron; for, wherever the treatment is alluded to, this remedy is included in the prescriptions. We congratulate the author on the marked success of his measures, which seem to have greatly limited his opportunities of *post-mortem* inquiry, so far as concerns his own clientele—one case only, where the steel treatment is said to have been used, is stated to have died. It was a young girl, aged 14, and it is obvious that her life was sacrificed by her own mad obstinacy. The author's *doctrine* upon this point is as follows:—

“The tincture of sesqui-chloride of iron has been so generally adopted by the profession, as to afford a guarantee that it has in the main been useful in their hands. There are, nevertheless, cases in which I have found chlorate of potash more useful; and there are others in which the combination of the latter with the tincture of iron is a better remedy than either of them separately.”

Our *practice* agrees with that of Dr. Greenhow, as shown in his cases. Our *doctrine* upon the steel treatment is in its favour, without any reserve whatever.

This treatise is a valuable addition to British medical literature. It will ever be a necessary book of reference on the history of diphtheria; and, in a practical aspect, deserves the perusal of all who are likely to be called upon to cope with the formidable disorder of which it treats.

*The Natural History Review.* A Quarterly Journal of Biological Science. London: Williams and Norgate. No. I. January, 1861.

THIS is a fresh issue, and a developed one, of the “Irish Natural History Review, and Quarterly Journal of Science;” and, as the Editors announce, is intended to be “A Quarterly Critical Journal of Biological Science, which, without interfering with existing scientific periodicals, shall stand in the same relation to naturalists and other persons interested in Biological inquiries, as that which is occupied by the ordinary quarterly reviews in respect of men of letters and the general public.” This is a large promise, and one scarcely likely to be fulfilled, judging by the first number. The principal paper in this Number is one “On the Zoological Relations of Man with [*sic*] the Lower Animals,” by Professor Huxley, F. R. S. This paper is well worth a careful perusal, notwithstanding the manifest bitterness of its author against his antagonist (Professor Owen), and is occupied with the discussion

of the classificatory position of Man among the *Quadrumana*, on evidence furnished by the anatomy of the brain.

Linnaeus ranked Man in the same order as the Ape, the Lemur, and Bat, founding his classification on an organ very different from the brain. Professor Owen, and several of the moderns, raise Man into the dignity of a Sub-class (*Archencephala*). Professor Huxley objects, as we think justly, to this arrangement; and joins issue with Owen on the three following points:—

1. The posterior lobe of the cerebrum, overlapping the cerebellum.

2. The posterior cornu of the lateral ventricle.

3. The *Hippocampus minor*.

The possession of these three peculiarities, according to Owen, is the prerogative of the brain of Man. Huxley denies that any of them is peculiar to Man.

The evidence against Owen's view, on the first point, is overwhelming, including the authority of Tiedemann, Cuvier, and what Professor Huxley maliciously calls "an authority which, I am sure, Professor Owen will regard as irrefragable," viz., Owen himself. He also quotes Macartney, Sandifort, Vrolik, Geoffroy St. Hilaire, Schröder van der Kolk, Gratiet, and his friend, Dr. Allen Thompson. He might also have added a higher authority than any of those named, viz., the monkey's brain itself, which, in all the species we have examined, overlaps the cerebellum fully. Professor Huxley spends much of his force on this first, and least doubtful, point, but is much less satisfactory in his attack on the *posterior cornu* and *Hippocampus minor*; his principal argument on the *posterior cornu* being a sneer at Owen for having borrowed a plate from another anatomist without acknowledgment; while with respect to the *Hippocampus minor*, his chief authority is the work of the Brothers Wenzel, who say, with respect to the "*Tuber in cornu posteriore ventriculorum laterali*um :—Non semper plerumque tamen adest, et quidem utroque in latere sive in utroque cornu. Inter quinquaginta et unum, eo specialiter fine a nobis examinata cerebra diversæ omnino ætatis atque utriusque sexus, tria tantum reperiēbamus in quibus tuber illud in utroque latere, et duo in quibus uno in latere considerabatur."

We have, ourselves, met with human brains, in which it was, to say the least, exceedingly difficult to demonstrate the existence of this hippocampic tuber.

On the whole, we believe Mr. Huxley has the facts on his side of this controversy, and we regret, therefore, that he has

shown so much personal feeling in the matter. Having pushed man from his *archencephalic* position, and reduced him to his proper level among the *gyrencephala*, he seems to think that he has made out a case for what he pedantically calls "the pithecoïd pedigree" of man. We decline to follow him into this region of poetry and speculation, which we do not believe to lie within the domain of science, and which is only conversant with what he calls "orders of facts, which are neither demonstrable nor discernible," and which are as remote from science and logic as the metaphysical theories at which he pretends to sneer.

The other papers of this number of the new Review do not fall within our province; but, on the whole, they appear to be rather heavy reading, not as attractive as the Quarterly or Edinburgh. Dr. M'Donnell's paper, "On the Electrical Organ of the Skate," is an old friend, the substance of it having been read some time ago before the Royal Irish Academy. In a paper on the *Axolotl*, the author's Latin strikes us as being somewhat peculiar, the word *ovæ* occurring repeatedly, in the sense of *eggs*; and the word *amphibia* being used as the singular nominative, agreeing, no doubt, with *animal*, understood: the author of this paper is a Master of Arts, of Dublin and Oxford; surely he ought to have known better; at least, as a naturalist, he should have been familiar with the Latin for *eggs*, from the maxim—

Omne vivum ex ovo.

The following passage would have delighted Cobbett as an example in English parsing:—

"Surely such embryological distinctions point to, at least, a difference in the general—in the ordinary acceptation of this word—of these creatures."

The editors of the Review are supposed to hold somewhat advanced views on natural science, and to follow Darwin and Huxley, *haud passibus æquis*; but it surely is not necessary to carry their ideas of progress so far as to invade our traditional notions on the subject of Latin declensions and English grammar. We hope we have seen the last of this slipshod writing.

*On Organic Polarity; showing a Connexion to exist between Organic Forces and ordinary Polar Forces.* By H. F. BAXTER, Esq., M.R.C.S.E., &c. London: Churchill. 12mo, pp. 187. 1860.

In this little work the author deals with some of the most difficult questions of rational physiology; and we gather from the note to page 56, that the Royal Society refused to publish his papers "on the development of current force during lacteal absorption, nutrition in the muscular and the nervous tissues, and in plants," read before that Society, in 1852. This book appears to be an abstract of those papers, and we believe it will repay a careful perusal; although we do not pledge ourselves to its results, as we think the questions it deals with are not yet removed beyond the pale of reasonable difference of opinion.

The following preliminary rules will be found useful to those about to read the book for themselves, as there is at least some obscurity in Mr. Baxter's statement of first principles, and his book requires some previous knowledge of the subject of Voltaic Electricity.

1. When oxygen, or an acid, acts on a metal, the latter assumes *negative* tension, while the oxygen, or acid, assumes *positive* tension.

2. When a liquid acid acts upon a liquid base, e. g. sulphuric acid on soda, the base, like a metal assumes *negative* tension, and the acid assumes *positive* tension.

3. If an electrolyte be placed, in the circuit, and *decomposed*, its particles are polarised oppositely to those of the *combining* fluid; its oxygen, or acid, appearing at the *positive* pole, and its hydrogen, or base, appearing at the *negative* pole of the circuit.

According to these statements the metal acted on chemically, the bases, &c., are *negative*, while the metal not acted on, the acids, oxygen, &c., are *positive*. Now, this is a matter of fact not depending on any hypothesis. How then comes it that language the very reverse is commonly used,—i. e. oxygen, the acids, &c., are called *electro negatives*, while the metals acted on, such as zinc, hydrogen, the bases, are called *electro positives*? This mode of speaking is the offspring of a theory of Grotthuss, who supposed that the poles or extremities of the terminal wires of a battery, one of which was in a state of positive tension, the other of negative tension, exercised attractive and repulsive actions on the constituents of chemical compounds, such as water, sulphate of soda, &c., the constituents themselves

being supposed to be endowed with the electric tensions of the two kinds. According to this hypothesis, accordingly, all the bodies, such as oxygen and the acids which are developed at the positive wire, are *electro negatives*; the others, such as hydrogen and the basis which are developed at the negative wire, electro positives. Mr. Baxter applies these or similar principles to the explanation of the following facts:—

(a). When the electrodes of a galvanometer are brought into contact, one with the mucous surface of the intestine in a living or recently-killed animal, and the other with the venous blood from the same part, an effect occurs upon the needle, which shows that the electrode in contact with the blood is *positive* to that in contact with the mucous surface.

(b). Under similar circumstances, the electrode in contact with the gall bladder is *negative* to that inserted into the hepatic vein, or vena cava ascendens.

(c). The electrode inserted into the renal vein is *positive* with respect to that inserted into the pelvis of the kidney.

(d). The electrode inserted into a lactiferous vessel, and that inserted into a vein from the same part, are in opposite tensions, the latter being positive.

Mr. Baxter insists, that in these and similar cases of secretion, the phenomena are best explained by electrolysis:—

“The arterial blood [or, blood supplied to a secreting organ] being, as it were, separated into its two elements, the *secreted product* and *venous blood* [or, blood leaving the organ] just as muriate of soda is decomposed and separated into its two elements, muriatic acid and soda.”

We believe this conclusion of Mr. Baxter to be larger than his premises, inasmuch as no chemical decomposition can take place in a voltaic circuit, without a corresponding, and *more powerful* composition also taking place in some other part of the same circuit.

The fact of the opposite tension of these secretions, and of the blood flowing from them, is important, but it admits of many and diverse interpretations. The same remark will apply to Mr. Baxter's explanation of other facts, such as—

(e). That, during nutrition in the muscular tissue, the muscular fibre and the venous blood are in opposite electrical states.

(f). That the blood of the internal jugular is positive with respect to the brain.

(g). That the external and internal portions of the muscles and nerves are in opposite polar conditions.

While we admit the full value of the investigations of Mr. Baxter and others on the electrical condition of the various parts of the body, we must be allowed to put in a plea for a simpler view of the matter; we believe all these electrical, calorific, motor, sentient, and even intellectual phenomena, are the results of the chemical changes the food undergoes before it is finally excreted; that such changes are accompanied by electrical, calorific, and motor effects, is now fully proved; and we believe more will be accomplished by physiologists in attending to the chemical phenomena of the organs of the body, than by directing their investigations to the electrical effects of those chemical phenomena which are, in reality, one step farther removed from the mystery of life, than the chemical actions are which produce them.

## PART III.

### MEDICAL MISCELLANY.

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#### TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY\*.

(Continued from vol. xxix., p. 488.)

SESSION 1860-61.

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OCTOBER 10, 1860.

DR. TANNER, PRESIDENT, in the Chair.

*Cancer of the Pylorus; jaundice; anemia.*—DR. FINN exhibited morbid specimens, exemplifying the above.

Ellen Haly, domestic servant, aged 35, unmarried, was admitted into the North Infirmary on the 30th April, 1860, complaining of irritability of the stomach, and palpitation of the heart. These symptoms dated from a year previous to her admission. Her general health had been good to that period, with the exception of the occasional occurrence of rheumatic pains, referred to the shoulders and lower extremities. Her mother had suffered from bad stomach, and died in consequence; she stated that the catamenia had disappeared about five months since, and that she has latterly been much troubled with costiveness; pulse, 90, feeble; tongue pale; and face anemic, with an expression suggestive of extreme anxiety. On examining the abdomen, a tumour of an oval shape, but presenting much irregularity of surface, was observed to occupy portions of the right hypochondriac and umbilical regions; when first observed, it was about the size of a small orange, but its volume subsequently increased. Manual examination, with a view to ascertain its outline, was attended with great pain; when pressed, it communicated a moderate impulse to the hand; and auscultation revealed a distinct arterial murmur, which varied in its character under the influence of pressure. A loud murmur was also heard

\* These Reports have been furnished to us by Dr. S. Henry Hobart, Secretary to the Society.—ED.



over the præcordial region, and over the chest generally; and the veins of the neck exemplified the auscultatory phenomena usually associated with spanemia. The treatment consisted of the frequent employment of enemata, necessitated by the costiveness (already noticed), and of the internal administration of the nitrate of bismuth, which latter medicine appeared to exert very considerable influence in controlling the irritability of stomach.

On the 4th of September, the surface became generally jaundiced, the jaundice having been ushered in by an aggravation, for some days previously, of the gastric symptoms.

5th. Irritability of stomach has increased much since yesterday, a large quantity of dark, grumous matter, having been frequently rejected during the night.

From this period to that of her death, on the 28th September, she suffered from acute pain, referred to the abdomen generally, and to the vicinity of the tumour more particularly, which pain increased in severity towards evening and night.

*Autopsy.*—On exposing the abdominal viscera, the tumour above noticed was observed to be situated near the free edge of the right lobe of the liver. The gall-bladder, a portion of the transverse colon, the head of the pancreas, and the pylorus, severally contributed to its production, having mutually contracted rigid adhesions at that point.

The point of departure of morbid change, however, appeared to be the pylorus, which was the seat of cancerous degeneration, the latter involving also the commencement of the duodenum. A circular portion of the pylorus, to the extent of a shilling, had been destroyed by ulceration; and its circumference, at every point, was united by adhesive inflammation to the fundus of the gall-bladder. The latter had thus, in a remedial capacity, combined the office of a mucous membrane with that of maintaining the integrity of the intestinal canal. The ductus choledochus was much enlarged; the mucous membrane of the stomach was pale, with the exception of the part immediately adjoining the pylorus. Towards the greater curvature were observed some air-bubbles, about the size of a pea, in the sub-mucous areolar tissue. The liver was congested, but not altered in its form, though somewhat diminished in volume; it offered an example of cirrhosis in its incipient form. The pericardium contained a small quantity of serous effusion. The heart was pale and small; at a short distance above the aortic valves, some atheromatous deposit was observed.

*Case of Pulmonary Apoplexy, with fibrinous clots in left ventricle, and partial Pericarditis.*—By DR. CUMMINS.

The following most painful case seems to me so interesting, that I shall not apologise for entering into its minutest details:—

Mr. H., aged 63, a short thickset man, of active, temperate habits, who had always enjoyed excellent health until the winter of 1859–60, caught cold during the very severe weather, and suffered

from cough for some time, without seeking medical advice. I was called to him on the 23rd of February, and informed that he had been much worse the last three weeks; complaining of palpitation, bad cough, and swelling of feet and legs, but no difficulty of breathing. He still persisted in refusing to call in a medical man until the evening I first saw him, when urgent dyspnoea suddenly set in, and obliged him to seek relief. His wife informs me also that for some time past his sleep has been much disturbed by frightful dreams. My first visit was paid at 9 p.m., about half an hour after the commencement of dyspnoea. I found him sitting up in the bed, gasping for breath, with an occasional dry cough at the end of expiration. Pulse regular, feeble, and so rapid that I was unable to count it. Heart's sounds quite inaudible, and no impulse to be felt; cardiac dulness normal; respiratory murmur heard loudly, without any prolongation of expiration, over every part of both lungs, mixed here and there with some dry and moist bronchial ronchi, but no dulness on percussion; respirations 48 in the minute. I administered ether, *sal volatile*, and chloroform, one after the other had failed, without affording any relief. I ordered an enema of turpentine, to remove some flatus which was present. Large mustard plasters were applied over chest, neck, and epigastrium. A strong emetic was given, and failed to make him vomit, even when repeated. He also got five grains of calomel; and a large blister was applied to the cardiac region. During all this time, I repeatedly examined the heart, without being able to detect the least sound. It was fearful to witness the agony of the orthopnoea without being able to relieve it, and to stand by, watching the failure of one remedy after another, and only able to obviate the dreadful exhaustion and sinking by large doses of stimulants.

Towards morning the lips were quite blue, the face pallid, and covered with cold perspiration, the pulse at the wrist scarcely to be felt; and still the dreadful dyspnoea continued, preventing the exhausted patient from lying down, or snatching a moment's repose. I now prescribed the ethereal tincture of lobelia, in ten-drop doses every hour, with stimulants; and before I left him there was a slight improvement. Shortly after being relieved, he was able, though still greatly distressed, to lie down.

Dr. Tanner visited him with me in the afternoon, about five o'clock, when we found that the dyspnoea had almost left him; that he had dozed a little during the day; and that the lips, although still blue, were improved. Heart's sounds still quite inaudible; pulse at wrist scarcely to be felt; but over entire cardiac region is now to be heard synchronous with respiration; a metallic tinkling, which can be traced downwards to the umbilicus, and is evidently propagated from the stomach. The bronchial rales have rather extended, but there is no increased area of cardiac dulness or other abnormal sign in any part of the chest. There has been some transient delirium during the day. We directed that the lobelia

should be continued, and ordered a mixture of ether and *sal volatile* every two or three hours.

At half-past seven o'clock I visited him again, and found him greatly improved in appearance; the heart's sounds quite distinct, but accompanied towards the base by a most marked friction. I immediately ordered three grains of calomel, with a minute quantity of opium, every third hour; and applied another blister over the heart, as the first had not risen well.

February 25.—Spent a good night, and seems better; friction not so intense, but no increase of percussion sound; an obscure endocardial murmur is heard toward apex. Ordered mercurial ointment to thigh, blistered surface, and armpits; half a grain of calomel every second hour. 2 P.M. Dyspnœa has again returned, but not so urgently as before. Bowels have been opened. 8 P.M. Continues much in the same state; friction gradually diminishing.

26th.—Was rather delirious during the night. Heart's action more extended, and friction indistinct; no marked extension of dullness. Pleuritic friction sound heard in some spots over anterior part of left lung. Bronchial rales rather increased, especially over right lung, where the vesicular murmur is also observed. Dyspnœa much less urgent; some distention of jugular. Urine scanty, and high-coloured; bowels open; pulse still almost imperceptible, notwithstanding the increased cardiac action. Continue powder as before; dry cupping over both lungs; omit lobelia. Ordered a mixture of ammonia, squilla, senega, &c., every third hour. 4 P.M. Pulse distinct, and rather hard; heart's action violent; dyspnœa very urgent. Omit the mixture, and take the lobelia as before; continue powder; to have a turpentine enema. 10 P.M. Dyspnœa less urgent.

27th.—Slept little during the night; very restless; expectorated during the night much sero-sanguineous fluid, which still continues, and has relieved the breathing. Jugular distended; friction, if present, very indistinct; heart's action tumultuous, and extended. Ordered 3 grains of calomel, 1 grain ipecacuanha powder, 1 grain powdered squills; one powder to be taken every third hour. 10 P.M. The expectoration has continued all day; pulse very rapid, but distinct; breathing better; countenance and lips very livid.

28th.—Sinking rapidly. Respiration failing; pulse very rapid, but distinct; mind wandering. Two hours before death he was gasping convulsively for breath, and died at 8 P.M., after a hard struggle. The mercury had no effect on the mouth.

*Post-mortem*—Twenty-four hours after death.—Muscles well developed; much adipose tissue; rigor mortis ordinary; lips perfectly livid. On opening pericardium, we found a considerable quantity of serous effusion, and a recent lymphic exudation covering the anterior part of right ventricle; the remainder of heart and parietal pericardium being free from it, no adhesion of any part. Heart somewhat enlarged, but firm, and its muscular fibre well developed; valves and endocardium generally healthy; left ventricle

full of fibrinous clots; right side of heart distended with black blood. There is some effusion into both pleural sacs, but no adhesion. Base of both lungs a complete non-crepitant mass of blood; remaining portion of both organs intensely congested, and infiltrated with blood and serum in many parts. Bronchi much congested, and filled with a sero-sanguineous secretion, such as had been expectorated. Liver somewhat enlarged. We were obliged to perform the *sectio cadaveris* very rapidly; and, having promised not to soil the bed-clothes, were prevented from examining the parts as minutely as we should have wished.

From the first moment I saw this patient, I made up my mind that I had to deal with no ordinary case of spasmodic asthma; and still, at first sight, it appeared as if the orthopnoea were caused by some lesion of the pneumogastric nerve—a belief which might have derived support from the impossibility of acting on the stomach by emetics. It occurred to my mind that a parallel might be drawn between it and the dyspnoea, disproportionate to the amount of bronchitis present, observed in influenza, and attributed by some pathologists to a diminution of the “vital activity” which the lungs derive from the pneumogastrics; for here the dyspnoea was certainly out of all proportion to the slight bronchitis. But section of the pneumogastric nerves, instead of causing rapid respiration, has been found to diminish the number of respirations considerably; and here they amounted to forty-eight in the minute; so it was at once evident that there was at least no diminution in the nervous supply. What, then, was the cause? My attention was early directed towards the heart for information. Its sounds were inaudible, and no impulse was to be felt; still the character of the dyspnoea was very different from the “respiratory distress” of fatty degeneration of the heart; while the history and symptoms of the case also tended against that view. The conclusion I at last arrived at was, that there was some obstacle to the circulation through the left side of the heart, with passive congestion and serous infiltration of the lungs; and this opinion was formed long before sero-sanguineous expectoration confirmed it. Notwithstanding the amount of stimulants taken, reaction was long in being established; and when, at last, it came, with it came a new complication of the case, viz., partial pericarditis. It is a question fairly open to discussion whether the symptoms when I first saw him were not due to the first stage of pericarditis, obscured as to its physical signs by the absence of the cardiac sounds during a period of collapse. My own impression is, that the first link in the chain of morbid action was the formation of the fibrinous clots in the left ventricle; the second, congestion of the lungs; third, pericarditis. And the suddenness of invasion by the dyspnoea is in favour of this view; as is also the fact that there was complete absence of the physical signs of pericardial effusion in the commencement.

OCTOBER 24, 1860.

DR. TANNER, PRESIDENT, in the Chair.

*Case of Cancer of the Gall Bladder, and neighbouring tissues.* By T. W. BELCHER, M. A., and M. B., Oxon. and Dub., L.K.Q. C.P.I. —Denis Donovan, the subject of the following remarks, died in the hospital of the Cork workhouse, Oct. 13, 1860. During a lingering and painful illness he had been under the care of Dr. Townsend, at whose request the body was examined after death:—

The patient was a man of about fifty years of age; had been a hard drinker in earlier life; and when under medical treatment, exhibited several symptoms of internal cancerous disease. The general emaciation was extreme, while the abdomen continued tumid, and in some places had a nodulated feel. The countenance gave expression to an appearance of intense anxiety. The two prominent symptoms were, constipation of the bowels, and ever-recurring sickness of stomach.

The treatment consisted in medicines, administered with a view to relieve both of these conditions, and nourishing diet. Purgative medicines, given by the mouth, were found to be useless, so that enemata were in perpetual use to overcome the first of the symptoms already referred to; while drinks of milk and lime-water, blisters over the region of the stomach; acetate of morphia, and watery extract of opium; and pills composed of trisnitrate of bismuth, with sulphate of quinia, were exhibited for a considerable time. Under the last-mentioned combination he rallied a little, but finally sank and died, as before mentioned.

The post-mortem examination was undertaken the following morning; when, beside the great emaciation already referred to, very considerable depression existed above and below both clavicles; the abdomen was enormously distended, and though only twenty-four hours after death, was quite green in colour. Great care was taken in dissecting the skin from that region; but so thin were the abdominal parietes, that the first touch of the knife penetrated the colon, when a large quantity of feculent matter, of the most offensive odour, shot up with much force, and continued to exude so long as the examination lasted. On opening the thorax, a most horrible discharge of fetid gas took place, so that all concerned had to leave the room instantly.

The lungs were dark and congested; the heart natural, and the thoracic viscera, in other respects, healthy. The liver was found of the natural size, and apparently healthy. The gall-bladder was replaced by a cancerous tumour—the entire contents of the lesser omentum being one cancerous mass. The posterior surface of the mass was nodulated, and contained gall-stones in numbers. The ascending colon was large, so large as to resemble the stomach in general appearance; the junction of its ascending with its transverse portion was involved in the disease, and so strictured that the finger

could be passed through it with difficulty. The right kidney was enlarged; its pelvis cancerous, and the whole involved in the diseased mass. The left kidney of its normal size, and in its usual position.

The diseased condition of the vital organs just enumerated must have been of long standing, and affords a striking instance of how long the human constitution can withstand the inroads of a mortal foe. The stricture in the great intestine evidently caused its distension, as, beyond the seat of the disease, it was of its natural size, and empty; in like manner with the stomach; thus accounting satisfactorily for the two main symptoms of the case—the constipation of the bowels, and constant sickness of the stomach. How the functions of the gall-bladder were performed, if at all, remains a question for the medical critic.

Dr. BELCHER also read the following case, and exhibited the pathological specimens taken from the subject:—William Casey, aged 60, married, a pensioner, who served some years in India, and was formerly of intemperate habits, was admitted into the hospital of the Cork workhouse under the care of Dr. Wm. C. Townsend, Oct. 6, 1860.

He complained of debility, and troublesome cough; felt palpitation at the heart for a month previous to admission; never had rheumatism; height, 5 ft 10 in., and wasted in general appearance; is troubled with dyspnoea, and has not any desire for food; his tongue is clean; legs swollen; bowels regular; kidneys act; but does not sleep; changes his decubitus from one side to the other with difficulty and uneasiness. Circulation feeble; irregular, and intermittent—almost imperceptible at left side. Chest everywhere clear on percussion; bronchial râles all over the lungs, with mucons expectoration more or less tinged with blood. The heart's action feeble, very rapid, with occasional regurgitation, but without any trace of murmur; no increase of præcordial dulness; jugular pulsations very evident at both sides. Ordered, tincture of opium, 12 drops; compound spirit of ammonia, half a drachm; water, 1 oz., for a draught; 6 oz. of wine, and beef-tea.

12th. Lies entirely on right side; pulse almost imperceptible in either wrist; biliary tinge all over the body; tongue clean and cold; urine scanty, and highly loaded with bile; heart's action feeble, and extremely rapid; swelling of left leg very much increased; extremities cold; complains of want of sleep. Repeat the nourishment.

13th. Presents the collapsed state of a cholera patient; no pulse to be felt at wrist; decubitus now on the back. Repeat 6 oz. of wine, beef-tea. Died at 2 P. M., being the eighth day after admission.

*Post-mortem examination.*—Oct. 15th. The chest was externally in good condition; and although the muscles were somewhat wasted, yet fat was found everywhere on them. On opening the thorax, both lungs—but especially the right—presented a highly congested appearance. The right pleura contained a large quantity of fluid, while the left lung strongly adhered to the walls of the chest. Its

apex was so firmly attached, that considerable force had to be employed in dislodging it from its position, and this was only effected by leaving parts of the pulmonary substance behind. The right lung, when cut into, appeared engorged, and its superior portion carnified to a great extent. The heart was firmly adherent to the pericardium, which latter was glued to the left lung. The pericardium was greatly thickened, and loaded with fat throughout its entire extent. The heart's chambers were dilated, but its muscular substance not enlarged; the latter had the fawn-coloured tint usually understood to belong to fatty degeneration of that organ. The valves, with the exception of some fatty deposits, were, in other respects, in good working order. The left lung was loaded with milary tubercles, except at its lower portion, where about one-fourth of its tissue was apparently healthy. The apex of the right lung was also studded with tubercular deposit, and had a cicatrix which did not appear to have been adherent to the pleura. The middle lobe presented a pneumonic appearance, while the lowest portion was tubercular.

*Remarks.*—In this case the congested state of the right lung seemed due to the constant decubitus on that side, while the system was greatly debilitated. The heart, on close comparison with another of healthy size, was not really much enlarged, while the presence of fat in its tissue was quite evident; nevertheless, the ventricles were greatly dilated. The firm adherence of the pericardium, as well to the heart as to the lungs, would seem to have resulted from a former diseased condition of the system; while the abnormal state of the parts examined defied any clear traces of the coronary arteries. The kidneys should have been examined, but time was only afforded to open the thorax; after which the body was removed by friends of the deceased.

NOVEMBER 16, 1860.

DR. O'CONNOR, EX-PRESIDENT, in the Chair.

*Case of Pneumonia and Fetid Abscess of the Lung.*—DR. W. P. BERNARD exhibited the lungs of a patient who had died in the workhouse hospital, and related the following particulars of the case:—

Matthew M'Carthy, aged 68, labourer, of intemperate habits, was admitted into workhouse hospital, under the care of Dr. W. C. Townsend, on 19th June, 1860, suffering from great debility and paroxysms of coughing, coming on with extreme severity at irregular intervals.

He states he enjoyed excellent health until the year 1831, when he suffered from influenza. From this time he has never been many days free from harassing cough, which has become much more constant and distressing within the past few months.

October 15th. Nervous temperament; countenance anxious, and livid; pulse 100, feeble, and irregular; respiration 40; tongue clean

and moist; bowels act fairly; urine scanty, and high-coloured; no desire for food; chest clear on percussion, anteriorly and posteriorly; bronchial râles scattered over anterior portion of both lungs; decubitus entirely on the back; is free from pain, but suffers from frequent cough and dyspnoea, coming on in paroxysms of alarming severity, leaving the patient bathed in perspiration, and completely exhausted.

23rd. Is suffering from pneumonia, having exposed himself to cold whilst heated after one of his fits of coughing; complains of pain in left side; circulation and respiration quickened; much fever present; entire of right lung clear on percussion; left lung anteriorly clear above, but marked dulness over root and base, with absence of respiration posteriorly; distinct crepitation heard; expectoration profuse and bronchitic. To have five grains of Dover's powder every four hours, and four ounces of wine, with beef-tea, throughout the day.

24th. Feels weaker; expectoration tinged with blood; bronchial breathing over both lungs; a blister to be applied over root and base of left lung posteriorly; powders to be continued, and wine increased to six ounces; no chlorides in the urine.

25th. Spent a quiet night; feels better.

26th. Pulse 100, very small, irregular, and feeble; expectoration less profuse, tenacious, with rusty tinge; tongue slightly coated; urine loaded with bile and lithates; no chlorides present. To have half an ounce of oil; wine and Dover's powder to be continued.

27th. Slept fairly last night; looks and feels better; pulse 96, feeble, and regular; sputa bronchitic, and free from rusty tinge; pneumonic symptoms have disappeared, leaving him in nearly the same state as before the attack of pneumonia; to have (at his own request), instead of the wine, one ounce of whiskey made into punch, three times in the day. Dover's powder not to be given more than three times daily.

28th. Pneumonic symptoms have reappeared; prune-juice expectoration; increase of dulness over left lung posteriorly; anteriorly, distinct crepitation above left nipple, extending upwards, midway to clavicle. Dover's powder to be continued every four hours, as at first.

29th. Cough still very troublesome; circulation 100; respiration 32; skin cooler; expectoration not so characteristic as on last report, but fetor from it and the breath noticed for the first time. Dover's powder to be continued, and wine six ounces, substituted for whiskey; refuses his beef-tea.

November 1st. Less cough; pulse 96; respiration 23; fetor from breath and sputa very marked; base of left lung much clearer posteriorly on percussion; distinct crepitation on inspiration and expiration. Continue Dover's powder and wine, and, in addition, a table-spoonful frequently of a mixture, containing—syrup of tolu, 3 ounces; Hoffman's anodyne and laurel water, of each 3 drachms; mucilage, two ounces, and water to make up an 8-ounce mixture.

November 3rd. Slept better; pulse 100; respiration 48; cough



still very distressing; sputa rusty, and very fetid. Treatment to be continued, substituting 3 ounces of wine for the spirit.

5th. Extreme prostration; pulse 116, regular, but very feeble; respiration 34; tongue dry and red; expectoration less tinged, but fetid. Continue wine and Dover's powder.

7th. Still weaker than last report; cough nearly disappeared; expectoration scanty, resembling very fetid pus, with patches of a sanious character floating on the surface.

9th. Died at 8 A. M.; having had no cough for many hours previously.

*Autopsy*.—Thirty hours after death. Body greatly emaciated; muscles of the thorax healthy, with some adipose tissue; immediately on raising the sternum, surface of lungs appeared perfectly healthy, but quickly and perceptibly collapsed, particularly at right side; left lung free from adhesions anteriorly and superiorly; posteriorly and inferiorly adherent to the pleura costalis, and to the pericardium (the latter being much thinned, and almost transparent); right lung slightly adherent superiorly, more firmly posteriorly, and to the diaphragm. From the extreme base of left lung a quantity of fetid pus escaped from an abscess—the sac of which would have contained a large-sized orange; posterior portion, inferior portion, and base of right lung were hepatized; bronchial glands very much enlarged.

*Poisoning by the Fumes of Charcoal.* By JOHN POPHAM, M. D., Physician to the North Infirmary, and to the Cork Workhouse. The following cases of poisoning by carbonic acid gas seem to present some features of interest:—

Five sailors, Danes and Swedes, were admitted into the Cork North Infirmary on February 10th, 1860, from the brig *Helgesen*, lying at Merchant's-quay. It appeared that the vessel was infested with rats, which the captain determined to destroy by charcoal vapour; accordingly, all the crevices of the hold were carefully closed, in order to confine the gas as much as possible to the lower parts of the vessel. The sailors, five in number, were obliged to occupy an ante-room to the captain's cabin, recently painted; three of them slept in the room itself, and two in a closet off the room,—the dimensions of the former being four feet by seven—those of the closet, barely sufficient to hold a bed,—the doors and windows of both being made air-tight for the time. At about 5 o'clock in the morning, the watchman heard moans proceeding from the ante-room; and on opening the door, he found the men in a state of complete stupor. On going into the captain's sleeping-room, which was better ventilated, he also was discovered insensible, but still capable of answering when roused. Medical assistance was sent for, and Dr. Wycherly and myself attended without delay. We had the men wrapped up in warm blankets, and brought on deck, all of them, except the captain, incapable of being awakened up from stupor; their mouths were covered with white foam; the lips livid; the teeth so spasmodically

closed, that it required a strong lever to force open the jaws, which shut with a snap, when the plug was withdrawn. Their faces were deadly pale; the eyelids closed, offering much resistance to every attempt to raise them, the pupils being insensible to light, and much dilated. Their breathing was very laborious and with stertor; the number of respirations about 8 or 10 in the minute. The pulse averaged about 50, and was feeble and flickering, not felt in the smaller arteries. The temperature of the surface of their bodies was very low, both the hands and feet feeling like marble. When the men were brought into the air, after about half an hour, with the help of remedial measures, they were so far revived from their deep lethargy as to show a slight return of consciousness when they were strongly roused.

We were limited in our remedies, in consequence of the early hour at which the accident occurred, and the want of medicines in the ship; but manifest benefit was gained in each case by assiduously rubbing the surface of the body, keeping hot bottles to the stomach and feet, stimulating the nervous system by means of sinapisms along the spine, and when the power of swallowing began to return, giving mustard emetics, which we found of use, both unloading the stomach and exciting the whole system. I need hardly say that great difficulty was felt in administering these remedies, as the teeth had to be forced asunder,—the men resisting our efforts, and showing an obvious dislike to be stirred, and a desire to be allowed to sleep on. After a couple of hours, consciousness was a little more restored; and as the ship was a place quite unfit for them, they were removed to the North Infirmary, and placed in a well-ventilated ward. At this time their state was as follows: the feet intensely cold, foreheads hot; the muscles supporting their heads quite relaxed, so that they had to be kept up by pillows; their faces either livid or purplish; but the bloated and distorted condition of the features at first evident had settled down into the calm of profound sleep, so that as they were ranged side by side, all similarly locked in deep slumber, the imagination could scarcely avoid conceiving them to be bound by some magic spell. In addition to the remedies employed, cold affusion was applied to their heads, and aromatic spirits of ammonia and camphor were freely administered, with hot coffee as strong as could be made. In a few hours reaction took place, the circulation rose; and flushing of the face, with strong throbbing of the carotid and temporal arteries, came on, which symptoms were kept down by antiphlogistic means. When they recovered consciousness, they complained chiefly of vertigo, and frontal weight and pain as if the head would burst, also of noises in the ears, weak sight, and other symptoms of perverted nervous function, loss of speech continuing some time, and succeeded by indistinctness of articulation, and obvious confusion of ideas. The secretion of urine was totally arrested in all the cases for a considerable time. It took from 36 to 48 hours to restore them to *full* consciousness, most of which time was passed by them in sleep, some of

the patients were longer than others in regaining it. On the third day the drowsy stupor had passed off, and on the fourth they were discharged well.

*Remarks.*—In the preceding cases we noticed the effects of poisoning by carbonic acid gas from two co-operating causes, viz., the non-renewal of the air in their sleeping apartment, and the direct inhalation of the charcoal vapour, which, notwithstanding its high specific gravity, must have pervaded the ship; as the captain, who slept in a separate cabin, did not escape its effects. Though the five sailors were exposed during the same period of time, and to the same amount of poison, the power of resistance varied in the respective cases, some being more heavily stupefied than others. The boy appeared more powerfully affected than the men, but recovered more speedily.

The effects of carbonic acid gas upon the system are more deadly than those which arise from the simple deprivation of oxygen, they are, as Copland has well shown, not those of mere asphyxia; but of actual poisoning. If we breathe an atmosphere of pure nitrogen or hydrogen, death ensues from a passive cause, namely, the want of the elements of respiration, whereas carbonic acid is an active agent. Animals immersed in pure carbonic acid die in half a minute, whereas in the other two gases they live much longer. Besides, carbonic acid when pure, causes spasm of the glottis, thus rendering inspiration difficult, if not impossible.

As to the psychological effects produced in the above cases, they were, as far as could be ascertained, much more of a painful than a pleasurable kind. It is stated that the sensations felt during drowning are not so distressing as our great dramatist represents them to be in Clarence's dream,—

“O Lord! methought what pain it was to drown,  
What dreadful noise of water in mine ears,” &c.;

but it is, at least, certain that the sensations experienced during returning consciousness are eminently painful. Those felt in cases of poisoning by carbonic acid are still more distressing, as being more prolonged. Body and soul appear to be weighed down by an insupportable incubus; the phantoms which flit before the imagination produce awe and terror: such were the sensations in the above cases, as far as they could be recalled; but they were so poignant, that the bare recollection of them was a subject on which the men avoided to dwell.

*Cases of Facial Paralysis.* Read by PROFESSOR O'CONNOR.—Paralysis of the portio dura of the ninth pair of nerves is of such frequent occurrence, and generally of such easy diagnosis, that it affords more interest to the physiologist than to the practical physician. Still cases are occasionally met with, in which the unguarded physician may be led into serious error, in not duly recognising the cause of the disease, and consequently underrating its danger

—an error the more likely to occur, from its generally mild character. The cases I am about to relate are many of them of frequent occurrence. Others of them are such as may be only seen in a practice extending over very many years.

The causes of this affection are very diversified; still they may be reduced to three heads,—symptomatic, organic, and mechanical. Under each of these heads, I will relate a few of the cases which have come under my notice. A lady, in the last months of pregnancy, had very confined bowels. They had not been affected for nearly a fortnight. In such cases we are prepared for great disturbance of the nervous system. She complained much of headach, vertigo, defect of vision. These symptoms she did not attend to; but on sitting to breakfast one morning, her friends were alarmed at seeing her mouth drawn to one side, when she attempted to laugh. A few doses of active purgative medicine removed the whole of these alarming symptoms, showing clearly that in this case the paralysis was purely symptomatic.

A child about fifteen months old was sent to me a short time since. The nurse stated that whenever the child cried, his left eye was forcibly closed, the right remaining in an open stare; and that the same eye remained partially open in sleep. It is not difficult to make a child cry in a physician's study, so I had soon an opportunity of witnessing the state of things referred to by the nurse. I observed that not only did the right eye remain open during crying, but that the mouth was drawn to the left side. The nurse stated that the same occurred whenever the child laughed. It was not difficult to recognise the disease as facial paralysis. Still I was not freed from serious alarm, fearing that the origin of the disease might exist in the brain, and be the precursor of hydrocephalus, of which an infant, belonging to the same parents, died a short time previously. In looking over the records of cases of this disease, I could not find one at this early age. After a most careful watching of the child for some days, I became satisfied that no other part of the nervous system was affected. At one of my visits the nurse announced a great improvement in the symptoms, which coincided in time with the appearance of a jaw-tooth. From this time forward the deformity became every day less, till it finally disappeared. In this singular case, the disease was clearly produced by difficult dentition. Under the head of organic causes, may be mentioned those cases which result from impressions on the extremities of the branches of this nerve by draughts of cold air, eruptive diseases, or by rheumatic inflammation of the neurilemma; although, in these cases, structural change in the nerve is more a matter of inference, than capable of demonstration.

A young female was under my care, suffering from an herpetic eruption on the right cheek. This receded suddenly, and was immediately followed by paralysis of the portio dura of the sound side, which was not removed for several months. It is more common to see the sensitive powers affected by suppression of her-

pes than a nerve of motion. However, both are met with frequently enough. A gentleman, who got out of his bed at night, was suddenly exposed to a draught of cold air from the window, and became seized next day with this affection. A patient, now under my care in hospital, became similarly affected from sleeping in the open air. In all these cases, we must believe that some structural change took place in the nerve. The third, or mechanical cause, is the more frequent, and is more or less free from or attended with danger, according to the situation of the pressure, as to whether it occurs within the cranium, in its passage through the bone, or external to it. Cases of the latter are seen after using the forceps in the delivery of a child. The deformity in these cases is sometimes transitory, sometimes permanent, in proportion to the amount of injury done to the nerve. A coachman received a blow of a sweeping-brush (accidentally) behind the ears. He was for some time insensible, and on his recovery was found to be affected with the ordinary symptoms of facial paralysis, without any symptom of cerebral disease. The disease in this case continued during life.

A case came under my notice in the workhouse, in which a considerable portion of the temporal bone was denuded by cancer. In this there was paralysis of the muscles supplied by the portio dura. A young female had caries of the petrous portion of the temporal bone. Abscesses formed, which burst externally, and dried up after some time, leaving, however, permanent deformity from injury to the portio dura in its passage through the bone. It is when the origin of the disease exists within the cranium, either from morbid growths, or the products of inflammation pressing on the nerve at its origin, that the greatest danger exists.

In these cases there is reason to apprehend that a further extension of the diseased structure may press on the brain, and produce fatal results. Some time since, in consultation with a member of this society, I saw a gentleman whose principal symptom was facial paralysis. He had, however, in addition, great pain in the affected side of the head, extending to the occiput, and some uneasy feeling on turning his head, which he avoided as much as possible. He was also somewhat deaf on the side at which the paralysis existed. We formed the opinion that the origin of the paralysis was within the cranium—perhaps disease of internal table of the occipital bone. He died after some time of cerebral disease, which was preceded by symptoms resembling caries of the odontoid process.

I had an opportunity of witnessing at the workhouse a rare form of this affection, namely, paralysis of the portio dura of both sides. The patient had been suffering for some time from secondary syphilis, especially from periostitis of the cranial bones. Paralysis of the portio dura occurred first on the left side, and after a little time the opposite side became affected. Still the hearing was not injured, nor the sensorium disturbed,—though to look at his countenance, he seemed to be perfectly idiotic. His features had lost all expression. The eyes were in a constant stare, bloodshot, and suf-

fused with tears, which also trickled down the cheeks. The angles of the lips falling down permitted the saliva to flow over, as also some portions of liquid, whenever he attempted to swallow, which was performed with difficulty. As he could not use the lips in speaking, his voice was guttural, coming as if from the back of his throat. As the disease was not supposed to be dangerous to life, his countenance produced more a feeling of ridicule than of pity among his fellow patients, which compelled him to leave the hospital before I could learn the termination of the disease.

I hope the recital of these cases may justify me in bringing under the notice of the society this trite subject, as it will be perceived that the disease is not always easy of diagnosis, nor trivial in its results.

*Hypertrophy of Heart; Disease of the Aortic Valves; Pericardial Effusion; Duodenitis; Jaundice.*—Dr. Finn communicated the particulars of this case, exhibiting at the same time the pathological specimens.

John Kelcher, aged 17, was admitted *in extremis* into the North Infirmary, on the 4th of November, 1860. He had been labouring under cardiac disease for more than a year; and the difficulty of breathing, from which he occasionally suffered during the earlier period of his illness, had latterly become so urgent as to preclude horizontal decumbency. He has also been much troubled with cough; but never had rheumatism, nor has he inherited any tendency to this disease. His employment, that of assistant in a tobacco manufactory, and excessive indulgence in the use of tobacco (which he both smoked and chewed), had probably contributed to aggravate, in no ordinary degree, the cardiac symptoms. Soon after admission to hospital, he complained of irritability of stomach, which was the precursor of an attack of jaundice. *General Symptoms*:—Loss of appetite, coated tongue, full, rapid, and bounding pulse; and œdema of lower extremities. *Physical signs*:—Visible pulsation of the arteries generally; cardiac fremitus; loud bellows' murmur, replacing both sounds of the heart. He died on the 16th of November, having suffered for some days previously, both from epistaxis and hemoptysis.

*Autopsy (Chest).*—The lungs were universally adherent; there existed considerable congestion in their inferior and dependent parts. The pericardium contained a large quantity of serous effusion. The heart, which presented a globular form, was greatly hypertrophied; the thickness of the parietes of left ventricle, at the base, centre, and apex, was nearly threefold greater than that of the normal standard. The mitral valves were much swollen, but in other respects healthy; the aortic valves were corrugated, and studded with deposit to such an extent, as utterly to preclude the performance of their office. The portion of the mucous membrane of the duodenum, contiguous to the opening of the ductus choledochus, was

deeply congested and thickened. The gall-bladder contained a large quantity of inspissated bile, and some biliary calculi.

The President read the following communication, which he had received from his friend Dr. Carey, of the Royal Artillery, stationed at Ballincollig:—

SIR,—My attention having been lately directed to the fact of ague existing to some extent in this neighbourhood, I have the honour to lay before you the result of some inquiries I have made relative to that disease. The subject is an interesting one, not only from the knowledge of ague existing to the extent that the numbers furnished exhibit (when we consider that Ireland is not held to be an aguish country), but also from the importance that such a circumstance has in connexion with the large body of troops constantly stationed in and about this neighbourhood. That ague does exist, to a very great extent, I am fully satisfied. The statistics furnished were supplied to me by an intelligent and practical physician, who has had long experience in this country, and ample opportunity of observing the disease, with its various exciting and predisposing causes. To me this is a peculiarly interesting topic, as, during my residence here, the troops under my charge have enjoyed a strange immunity from ague, while around them on every side the disease has made its havoc.

Before I enter on the subject of the disease itself, I deem it necessary to furnish you with as much of the topography of this district as I can, such being the key to the right understanding of the nature and habits (if the term can be applied) of this disease.

From the city of Cork to about twenty miles west of Ballincollig, the country is divided into two districts by the River Lee. To the north it is hilly, the soil fertile, with a substratum of *brown sandstone*. There is a good deal of marsh between small hills. There is a large lake in the neighbourhood of Blarney, about four miles from this place; and the country about Blarney is marshy, and intersected by many running streams. In winter, several acres are entirely covered with water. One hill, about five miles to the N. W. of this barrack, is higher than all the rest, and on this hill more ague has occurred than in any other part of the country. There is a good deal of wood at this side of the River Lee. To the south of the river, the country is much more flat for some distance, very marshy; and this marsh extends, in a long vein, for several miles to the west. To the left of this marshy vein, there is a range of low hills, and from these numerous rivulets flow into the flat district under them. The soil here is in parts fertile, but the substratum of stone is *limestone*. This is of two kinds—one hard, mixed with black marble, the other *dolomite*, very acrid and burning when in a state of mud. To the west, the country is wild, and badly cultivated; and about one mile from the barrack there is a pond on the road-side, which I am informed is a fertile source of ague in the spring; at least, the disease in that neighbourhood increases consi-

derably when the water subsides in this pond. To the *east*, and towards Cork, there is a long flat district, a continuation of the marshy vein I mentioned before; and included in this vein is the western suburb of Cork, a spot bearing the strongest features of an aguish district, and in which I saw a very bad case this summer. It occurred in a house situated on the edge of what is called "the Dyke"—a stream running between a marshy field leading down to the River Lee, and a gravelled promenade running parallel to the public road, which is about fifty yards distant.

This barrack is situated about five miles to the west of Cork, about midway between the range of hills to the north and the flat vein of marsh to the south I before mentioned. Of its internal structure you are already fully aware; and although its deficiencies, in some sanitary respects, require amendment, still you will agree with me in thinking it a strange fact, that within a period of a year I have had but one case of ague, and that occurred in a constitution peculiarly predisposed.

In connexion with the barrack is a large powder-mill, extending over several acres. The various small mills constituting it are supplied with water by canals, running in every direction. There is a good-sized village in the enclosure, inhabited by the families connected with the working of the mills. The village is within four hundred yards of the outer wall of the barrack, and among the inhabitants of this village ague runs its course to a very great extent. There is a point which I deem of much importance to notice, when speaking of the marshy nature of some of the country about here; it is commonly called *boggy*, but it is not a peat country. There is no turf or peat cut in the country for many miles round; and it will be shown by a comparison with other parts of Ireland, where there is quite as much wet ground, that, as far as we know of the disease ague, this circumstance is of much interest.

The *form of ague* principally seen here is the tertian; some cases of quotidian have been seen, also several cases of remittent fever among children, and some of these while at the breast. They are all of a severe type, but have been generally treated with success. They occur at all months of the year, but principally in March, April, and May. I have been told that when easterly winds prevail, ague is worst, and most frequent; but this year the prevailing wind has been N. W., and this has been the worst year for a long time. Ague has been increasing of late years. The Table annexed will clearly point out these circumstances.

It is not for me here to attempt any general disquisition on the real nature of the poison that gives rise to this distressing disease in many quarters of the world; it has been met with, ably discussed, and now almost entirely under the control of a few specifics. The influence of wet soils, decaying vegetable matter, certain prevailing winds, &c., are well known; but there is one point that, perhaps, is not so much thought of. Many years ago, while I was a student, I heard an able physician in Dublin (Professor Osborne)



state that he thought the absence or presence of a peaty soil had much to do with the prevalence or otherwise of ague in Ireland; and my own experience, and the account of even unsophisticated people, have strongly impressed me with the value of the assertion.

For nearly twenty-two years I resided constantly in the vicinity of a large peat bog—one that supplied turf to many parts of the Upper and Lower Shannon. While walking through many marshy districts near this bog, the peculiar dark-coloured water exuded proved the presence of peat; and it was so strongly impregnated with the colouring matter contained in it, as to leave a permanent stain in clothes, and even in the skin of the feet, which could not be got rid of until the cuticle came off. I have frequently remarked the astringent taste of this bog-water—of its preservative and antiseptic quality there is no doubt. I have seen animal substances, such as butter (which has been buried in a bog for two years), taken up perfectly sound, and have been told of bodies exhumed after a long period almost unchanged in condition.

It has been suggested by a well-known author, that the existence of "tannin" in peat-bog was, perhaps, the cause of the absence of ague in its neighbourhood; but whatever the cause may be, I have not seen, for the twenty-two years I lived near one, a single case of idiopathic ague, and for ten years of that period I was constantly engaged in assisting in a large dispensary district.

A strange fact was mentioned to me a short time ago by a non-professional gentleman. In a district on the Upper Shannon, one side of the country is a peat country. The River Shannon flows between this and the other side, which is marshy, but has no peat; and the marshy side is, as he said, "full of ague," while the other side is said not to have any. That ague selects for its victims those who are debilitated, and whose nervous systems are unusually depressed, is, I believe, generally received. There can be little doubt that the poor of this district are readily susceptible of it—badly clad, badly housed, and badly fed. The mud hut and clayey floor exhaling constant damps, and above all the filthy cess-pool at the door,—the plague spot of an Irish hovel,—must lay them open to a disease whose favourite nidus is found in damp places devoid of a circulation of air.

An officer, who had been a good deal in Canada, told me he could distinctly recognize the kind of atmosphere where ague existed, and could predict the occurrence of ague by a peculiar smell in the atmosphere, known best to those who have once perceived it. A gentleman in this neighbourhood got concussion of the brain, not long ago; and when debilitated by the attack, and consequent confinement to the house, had a sharp attack of ague, to crown all; and in my own practice, a patient was liable to the disease for a period of two years, who had been subjected to repeated courses of that depressing drug, mercury, and did not lose the disposition to it until he went to a hot and dry climate.

I may again recur to the infrequency of ague in this barrack;

and may in all fairness ascribe it to the difference in the circumstances and condition of the troops, contrasted with those around them; the good food, good clothing, regular habits, and exercise of the men must be the best prophylactic to a disease on the watch for the reverse; and though occasionally, in other climates, soldiers do fall victims, the causes might be found in some depressing circumstances, such as a long march previous to encamping in an agueish district, and that too at night; or a deficiency in the supply of proper food, or wisely-administered stimulant.

Year Commencing January, 1854.

1854.	Ague.	1858.	Ague.
January, . . . . .	None.	February, . . . . .	6
February, . . . . .	10	March, . . . . .	21
March, . . . . .	16	April, . . . . .	40
April, . . . . .	13	May, . . . . .	36
May, . . . . .	9	June, . . . . .	21
	—	July, . . . . .	14
Total, . . . . .	48	August, . . . . .	8
		September, . . . . .	2
		October, . . . . .	4
		Total, . . . . .	152
1855.	Ague.	1859.	Ague.
January, . . . . .	None.	January, . . . . .	1
April, . . . . .	20	February, . . . . .	7
May, . . . . .	7	March, . . . . .	8
	—	April, . . . . .	28
Total, . . . . .	27	May, . . . . .	38
		June, . . . . .	22
		July, . . . . .	3
		August, . . . . .	5
		October, . . . . .	1
		December, . . . . .	1
		Total, . . . . .	114
1856.	Ague.	1860.	Ague.
April, . . . . .	18	January, . . . . .	6
May, . . . . .	22	February, . . . . .	2
June, . . . . .	7	March, . . . . .	15
July, . . . . .	21	April, . . . . .	41
August, . . . . .	4	May, . . . . .	51
September, . . . . .	3	June, . . . . .	15
	—	July, . . . . .	8
Total, . . . . .	75	August, . . . . .	3
		September, . . . . .	11
		Total, . . . . .	152
		Grand total, . . . . .	663
1857.	Ague.		
February, . . . . .	3		
March, . . . . .	14		
April, . . . . .	13		
May, . . . . .	27		
June, . . . . .	16		
July, . . . . .	13		
August, . . . . .	5		
September, . . . . .	4		
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Total, . . . . .	95		

JANUARY 9, 1861.

DR. WALL in the Chair.

DR. W. C. TOWNSEND brought forward the following case:—

James O'Neil, aged 25, was admitted into South Charitable Infirmary, December 6, 1860. Patient, a labourer, states that three years ago, when mowing, he spit a small quantity of blood, and that he has done so since frequently. His health had been good previously; but since these attacks came on, he has found himself getting thinner and weaker, and has been much troubled with cough. He was admitted into hospital labouring under a severe attack of hemoptysis. The symptoms were, frequent cough; expectoration bright red, nearly all blood, only a small quantity of mucus. Right side of chest expands better on inspiration than the left; more extensive dullness on percussion at the apex of the left lung than at that of the right. Bowels confined; appetite bad; decubitus easy in any position. Ordered four ounces of Epsom salts, half an ounce of dilute sulphuric acid, sixteen ounces of distilled water. Make a mixture. One wineglass full to be taken every three hours till it operates. Five grains of mercury with chalk to be taken at bedtime.

December 7th.—Unable to sleep last night, from the frequent cough. Spit three cups-full of blood; complains of slight headache. Continue medicine.

9th.—Hemoptysis checked; expectoration only slightly tinged; bowels free; no head-ache; pale countenance; pulse 102; slept most part of the night. Omit the acid saline mixture. Take of dilute sulphuric acid, two drachms; simple syrup, one ounce; water, eight ounces. Mix. Let him have one ounce every three hours. Repeat the grey powder every night.

11th.—Cough troublesome; no appearance of blood in sputa, which is very purulent and copious; bowels regular; free from pain anywhere; pulse 102.

12th.—Seems rather stupid this morning, and comprehends what is said to him with difficulty; feels his back stiff, and is unable to lean forward. Same in other respects. Omit the sulphuric acid mixture. Let him have one ounce of citrate of potash mixture every three hours. Repeat the grey powder at night.

13th.—Very heavy and stupid this morning; has a strong typhoid smell; tongue brown, and loaded; passed urine and feces involuntarily in bed; bowels very loose; complains of no pain; decubitus on either side. Ordered an enema of starch, with two scruples of laudnum. Half a pint of port wine at intervals during the day. Evening. Very weak, and comatose; taking a considerable time to take in his tongue after protruding it; hard to be roused to consciousness, immediately relapsing into stupor. Ordered a blister to the back of neck, blister over the region of heart if necessary; hot stupes to the feet; eight ounces of port wine.

14th.—Tongue very loaded and brown; stupor increased; roused with more difficulty, relapsing immediately into a comatose state. Discharged his stomach frequently, and does so after taking any drink; bowels free; passes urine and fæces in bed; very heavy typhoid smell. Was moved into a private ward. Continue two ounces of wine every third hour, and one ounce each of brandy and soda-water every second hour. Evening. A catheter was passed, and ten ounces of urine drawn off. Bowels moved twice in bed; stupor increased.

15th.—Breathing stertorous, and all the other symptoms increased; bowels act unconsciously; kept up with brandy; the stertor increases, and a very heavy typhoid smell. Sank rapidly since 9 A.M., and expired at 2 A.M. next morning.

*Autopsy.* Twenty-one hours after death.—All the vessels of the brain much congested; slight serous effusion at base; falx cerebri inflamed, and adherent to the brain. The substance of the brain was studded with small abscesses, and softened tubercles. The ventricle was healthy, and so was the cerebellum.

This man was brought into the South Infirmary, suffering from a severe attack of hemoptysis, consequent on advanced tubercular disease. Owing to the arrival of his friends, it was impossible to examine his chest or abdomen.

The question to be determined was, the cause of death. Did the patient die of pyæmia; or could he in any way have contracted the poison of typhus?

*Cases of Gastric Ulcer.* By DR. CUMMINS.—Drs. Brinton and Budd, in their valuable works on diseases of the stomach, have dwelt much on the danger of roughly handling the epigastrium, when there is reason to suspect disease of the stomach; and I lay the following short communication before the society, principally with a view to illustrating this point.

On the 1st of August last, I was called to see an elderly lady, who had for many years been the subject of gastric disease, and had been under the care of some of the first physicians of the day at various times. She had occasionally vomited small quantities of blood; and had more or less, throughout the entire period of her illness, suffered from a gnawing pain at the pit of the stomach, and corresponding part of back, increased by eating meat, and various other articles of food, as well as by mental emotion. The bowels have always been costive, requiring the daily use of a lavement; but in other respects the general health has been good, and her habits have been remarkably active. There is nothing in the appearance to create suspicion of malignant disease; and as vomiting has never been a symptom of her complaint, I concluded that the orifices of the stomach were not involved in any disease that might be present. She has also been at all times free from acute, or darting pain.

The fortnight previous to my first visit had been one of more suffering than usual. She was still, however, in the intervals of pain, able to go about, and join the family circle at meal-times.

On careful examination of the epigastrium, I discovered a tender spot, about its centre, corresponding to the situation of a small tumour, which, however, was not very distinctly perceptible.

When I stated to her my belief that she was the subject of ulceration of the stomach, she did not disguise her incredulity; and, to prove that I was mistaken, commenced thumping the part with her hands, and continued doing so for some minutes, notwithstanding my most urgent remonstrances. The consequence was, that almost immediately after, she suffered from a most violent paroxysm of pain, which subsided in about ten minutes, leaving her much in the same state as before. I prescribed a mixture containing bismuth and morphia, which, however, was not taken, as she felt better in the evening, and early part of next morning.

I fortunately stated to her friends my apprehensions of a fatal termination at some time, from either perforation or hemorrhage; but I was not at all prepared for the rapidity with which my words were to be verified. Early next morning, I received an urgent message; and as the distance was considerable, did not arrive for nearly two hours after, when I was informed that she had risen early, in her usual health and spirits, with the intention of going to town; that she had partaken of an excellent breakfast, and gone to her room to dress immediately after, when her maid, hearing her bell ring violently, came in, and found her suffering from agonizing pain in the stomach, accompanied by vomiting and faintness. At one glance I saw what had happened. There was no mistaking the collapsed expression, the knees drawn up, and hands extended to prevent any one touching her, the tympanitic distention of abdomen, the rapid thready pulse of 160, and all the other symptoms of peritonitis from perforation. I immediately sent for Dr. Meade, and, in concert with him, prescribed a grain of opium every hour, which had the effect of mitigating her sufferings, and prolonging her life until past midnight, when she sank gradually.

It was very unfortunate that no post-mortem could be obtained, not that there could be any doubt as to the cause of death, but to find out whether perforation had taken place at the time she so violently thumped the epigastrium; and if it did so, by what mechanical contrivance nature prevented effusion into the peritoneum, until after a full meal had distended the stomach.

I think every one who has heard my description of the history of the case, written at the time of the occurrence, will believe with me that the rough handling of the epigastrium was connected with the perforation, whether the latter occurred at the moment or not. We have all seen or read of the wonderful mechanical contrivances by which effusion of blood is sometimes temporarily stayed, after the bursting of aneurisms; and we can easily believe that something of the kind postponed the fatal effusion here.

It is probable that an ulcer, or ulcers, had been in existence in her stomach, alternating perhaps with periods of cicatrization, for many years; and that during the fortnight previous to my visit, which, as I already stated, was marked by great increase of pain, the ulceration had extended to the peritoneal coat. It is possible, also, that cancerous matter might have been latterly deposited in the "hard brawny mass" that generally surrounds those ulcers, and given rise to the tumour, indistinctly felt, or possibly it might have been caused by adhesion of the stomach to some neighbouring viscus.

The diagnosis of ulcer of the stomach is at all times involved in some obscurity; but when long-continued gastric disease is present, accompanied by hematemesis, and the peculiar gnawing pain of epigastrium and back, increased immediately after taking food, all of which were met with in this case, we have a group of symptoms, quite sufficient for a decided diagnosis; but when these were followed by peritonitis from perforation, there could be no doubt whatever as to the lesion that had existed during life. Had this lady lived a little longer, I should have put her on a systematic course of diet, such as Dr. Brinton recommends, with some hope of amelioration, if not of recovery, by cicatrization. I have seen great benefit result from such a course of treatment in similar cases; but unfortunately there is a great tendency to relapse afterwards, from the least imprudence in diet.

*Anal Orifice within the Labia Majora, separated from the Vagina by a Mucous Septum.* By H. P. BROWNE, M. D., K. & Q. C. P., and L. R. C. S. I.; Physician to Delgany Dispensary.

On the 18th December, 1860, I was requested to visit A. E., aged 35, the wife of a farmer residing in this district. On my arrival I found the woman collapsed, and pulseless, from excessive hemorrhage. Having ordered stimulants, and restored heat, I learned from her that she feared it would be a premature confinement. I then made an examination *per vaginam*; and finding an abnormal condition of parts, I requested the midwife in attendance to raise the bed-clothes, in order to satisfy myself by an ocular examination. To my great surprise, I found the termination of the rectum situated within the labia, and in close approximation to the vagina, separated from it merely by a mucous septum; so that you could pass the fore and middle fingers into the two openings at the same time.

I found the hemorrhage in this case controlled temporarily by the exhibition of the tincture of larch bark, as recommended in other cases by Dr. Frizelle.

On the 20th, strong abdominal pains set in, and the woman aborted, having expelled a fœtus of four months.

This patient is the mother of four healthy children. She states her labours have been always very tedious, doubtless arising from obstruction caused by the abnormal position of the rectum.

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*On the Preparation of Chloride of Zinc in Cylinders.* By M. SOMMIÉ.

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These pencils remain solid, of uniform causticity, and are easily manipulated. They act like a sponge, slowly exuding chloride of zinc, and liquify when in contact with the air or the skin.

For Canquoin's paste or chloride of zinc in slabs, gluten is preferable to flour as an excipient; it is more elastic and less hygrometric.—*Bulletin Général de Thérapeutique*, 15 June, 1860, p. 499.

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1. The British and Foreign Medico-Chirurgical Review and Journal of Practical Medicine. Published Quarterly. London: Churchill. (Received regularly.)

2. The Edinburgh Medical Journal. Published Monthly. Edinburgh: Sutherland and Knox. (Received irregularly.)

3. The Retrospect of Medicine, being a half-yearly Journal, containing a retrospective View of every Discovery and practical Improvement in the Medical Sciences. Edited by W. Braithwaite. London: Simpkin and Co. (Received regularly.)

4. The Half-Yearly Abstract of the Medical Sciences, being a practical and analytical Digest of the principal British and Continental Medical Works, &c. Published Half-Yearly. Edited by W. H. Ranking, M. D., and C. B. Radcliffe, M. D. London: Churchill. (Received regularly.)

5. Pharmaceutical Journal and Transactions. Published Monthly. London. (Received regularly.)

6. The Lancet. A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News. Edited by Thomas Wakley, Surgeon. Published Weekly. London. (Received regularly.)

7. Medical Times and Gazette. Published Weekly. London: John Churchill. (Received regularly.)

8. Association Medical Journal. Published weekly. London: Honeyman. (Received regularly.)

9. The Medical Circular. Published Weekly. London: Harris. (Received regularly.)

10. Medical Critic and Psychological Journal. Edited by Forbes Winslow, M. D., Published Quarterly. London: J. W. Davis. (Received regularly.)

11. The Asylum Journal of Mental Science. Edited by J. C. Bucknill, M. D. London: Longmans. (Received regularly.)

12. The Glasgow Medical Journal. Published Quarterly. Griffin and Co. (Received irregularly.)
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## INDIA.

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## AUSTRALIA.

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## AMERICA.

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23. The American Journal of Dental Science. Edited by C. A. Harris, M. D., and A. S. Piggot, M. D. Published Quarterly. Philadelphia: Lindsay and Blakiston. (Received regularly.)
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## FRANCE.

25. Gazette Médicale de Paris. Published Weekly. Paris. (Received regularly.)
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27. *Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue des nouvelles, scientifiques, nationales et étrangères, &c.* Published Monthly. Paris: Labé. (Received regularly.)

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29. *L'Union Médicale, Journal des intérêts scientifiques et pratiques, moraux et professionnels du Corps médical.* Published three times a Week. Paris. (Received regularly.)

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PART I.  
ORIGINAL COMMUNICATIONS.

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ART. VII.—*Remarks on the Semeiological Value of the Pulse in Childbed.* By ALFRED H. M'CLINTOCK M. D., F. R. C. S., President of the Dublin Obstetrical Society, Hon. Fellow of the London Obstetric Society, Vice-President of the Dublin Pathological Society, Master of the Lying-in Hospital, Dublin, &c.\*

THIS paper does not by any means pretend to be an essay on all the changes and modifications which the pulse undergoes in the physiological and pathological states of childbed: its object merely is to point out some cases which form exceptions to the general rules bearing on the interpretation of the pulse of puerperal patients.

These general rules, briefly stated, are, 1st, that where the frequency of the circulation does not exceed 80 in the minute, the patient may be considered as going on favourably; and, 2nd, that a pulse at 100, or upwards, indicates the presence of danger, or at all events of some mischief.

The careful observation of the condition of the pulse is

\* Read before the Association of the King and Queen's College of Physicians, March 6, 1861.

a duty of primary importance in the case of the childbed woman, and never should be omitted. Indeed, I venture to affirm that of all the symptoms belonging to the puerpera, taken singly, the pulse is the most valuable and the most reliable. Nevertheless, even the pulse may deceive the practitioner; and the present communication is intended to point out a few of the occasions where extreme circumspection is necessary before forming an opinion from the character of the pulse.

Of the different characters of the pulse as to *rythm*, *force*, and *frequency*, I have been accustomed to attach most importance to this last, viz. the rate of frequency. The other two, however, will never be lost sight of by the practitioner, as the force or strength of the pulse will often decide serious questions of treatment. Frequency alone will sometimes do the same thing; for a pulse above 120 or 130, no matter what its other characters may be, generally suggests extreme caution in the employment of depletory measures,—as much so, indeed, as a weaker pulse at 100 or 110. The late Dr. Todd has given the opinion—and all practical physicians must concur in it—that there is no one symptom so important as the pulse. But in his estimation, it is not so much the frequency, as the volume, the quality of the pulse, which bears a direct ratio to the vital powers of the patient. From the context it would seem as though he intended this observation to apply to cases of fever only; and if so, I believe his proposition will be generally admitted. To ascertain the frequency and rythm of the pulse is a simple matter, and can always be absolutely determined; but to form a correct opinion of the qualities comprehended under the term force or strength, which includes volume, compressibility, &c., requires great experience, and a very delicate tact.

The rules ordinarily laid down for taking the pulse—such as not doing so till the excitement caused by our first saluting the patient has subsided, and not reckoning the pulse for a shorter period than a quarter of a minute—apply with even more cogency in the case of the puerperal patient.

In a large proportion of the cases of normal convalescence from parturition, the pulse scarcely deviates at all from the standard of health. With the reaction succeeding to the shock of parturition, and with the coming of the milk, there may be a rise of a few beats; but even these temporary accelerations are more frequently absent than present. I continually meet with cases where the pulse does not exceed 72 all through childbed; and many other cases I have met with where it was as low as 60, 54, 48, and even 44. This morbid slowness was constitutional, I believe, and did not depend on any condition

peculiar to childbed. The late Dr Montgomery met with some cases of this kind; and my observation entirely agrees with his, that these women recover well. In none of them could I find any cause for the remarkable torpor of the circulation\*.

If the patient's pulse be 80 or 84, she is considered to be going on favourably; and, as a general rule, this inference would be correct. Let me point out, however, some cases where this reasoning would be fallacious, and where, though the pulse is quiet, considerable danger may be present, or, at all events, the patient's position may not be at all so satisfactory as the rate of the pulse would seem to indicate.

It has been thought by Beck, Hardy, and other observers, that where ergot has been given to the extent of two or three drachms, it may be followed by a depression in the rate of the pulse, lasting for two or three days, which might mislead us as to the true condition of the patient. The diminution so produced, however, is observable on rare occasions only, and seldom, I believe, amounts to more than ten or twelve beats in the minute; but this much would make all the difference between a normal and abnormal rate of frequency—that is to say, a pulse which but for the ergot would have been 96, may be only 84 in the minute.

I have frequently had occasion to observe—and no doubt so have many other obstetricians,—that in the commencement of metritis, there is oftentimes a definite period during which the vascular system shows no obvious sympathy with the local influence—the pulse not ranging above the ordinary standard. This tranquillity of the circulation does not last long; a few hours will find the patient in a state of general vascular excitement, with a hot skin, and a pulse at 96 or 100.

Here the local symptoms of inflammation are apparent for some hours before the pulse gives any unequivocal indications of the mischief that is going on, and hence the importance, I may incidentally remark, of always examining the condition of the uterus in the puerperal patient. With regard to the order of sequence of rigor, local pain or tenderness, and vascular disturbance, in cases of uterine or peritoneal inflammation, considerable differences are observable. The rigor may be al-

\* The most remarkable example I have seen of depression of the pulse was in a patient delivered of triplets. She was a thin delicate woman, and had come to the full time; the pains were energetic, and her labour most rapid; and such was the degree of collapse succeeding to it, that the pulse fell to 80, and I scarcely thought she would have survived: yet she recovered, however, and bore children subsequently. This woman had not naturally a slow pulse.

together wanting, and, as a general rule, these are the least formidable cases; or it may precede the other two symptoms, or it may follow them; or, again, disturbance of the circulation may take the initiative, and in the course of some hours, we find local symptoms of inflammation to make their appearance.

From paying too exclusive attention to the pulse, and under-estimating local symptoms, the attendant might be led into a culpable inactivity of treatment, and thus allow the disease to gain considerably ahead of him before employing sufficiently energetic remedial measures; and this error I myself more than once committed, at the outset of practice.

In cases such as I have been describing, although the rate of the pulse may not point out the presence of any lesion, yet the educated finger will seldom fail to observe a sharpness or quickness in its beats which is not the accompaniment of a healthy pulse.

Uterine tenderness may, no doubt, be due to other causes besides inflammation; but where the practitioner has any misgiving on this point, he should see the patient at short intervals, and thus, by vigilantly watching the course of events, adapt his treatment to the nature and exigency of the case.

A rigor may come on at any time in the course of an attack of puerperal fever (though it more commonly is one of the initiatory symptoms); and it not unfrequently happens that for some time,—I cannot say for how long exactly, but I think only an hour or two—previously to the fit of shivering, there is a marked diminution in the rapidity of the circulation, leading the attendant, if he should happen to see the patient at this period, to believe that the disease is yielding, when in truth it is gaining strength, and, it may be, getting beyond the reach of art. This pleasing delusion is soon dispelled, to the intense disappointment of friends, and the great mortification of the doctor, who has to recall the sanguine opinion so recently expressed, and to substitute one of grave foreboding. A knowledge of the fact just stated, obviously suggests the necessity of caution in drawing a prognosis from the character of the pulse at one visit. This precept is strictly in accordance with a maxim well known to practical accoucheurs in regard to puerperal fever, namely, not to regard any amendment in the patient's condition as real and genuine, unless it continue for twenty-four hours. The reason for this precept is, that a remission in the symptoms is apt to occur in the course of this disease, and may deceive even the most experienced practitioner, if he judges from the present condition of the patient, and without waiting for the test of time.

As the pulse may be lowered by an approaching rigor, so it may be reduced in frequency by the presence of nausea. This may be accidentally induced by medicine, or be a symptom of the puerperal disease; in either case, the nausea may depress the pulse, and should therefore be taken into account, when estimating its semeiological importance.

In the progress of cases of pyæmia, rapidity of pulse is a constant, indeed, almost an unfailing, attendant; yet even in those running on to a fatal issue, I have sometimes seen the pulse temporarily fall so low as 70 or 80,—not on the approach of a rigor, but after the sweating stage succeeding to rigor, and apparently the effect of extreme exhaustion. I need do no more than barely allude to the possibility of the circulation being also lowered in such cases by the action of some medicine administered to the patient; opium in large doses, or digitalis, are those most likely to have this effect. Of the *veratrum viride*, so highly extolled for its sedative effect by American writers, I have no experience.

Having thus mentioned some of the qualifications with which we are to receive the maxim that “a pulse at the normal rate is evidence of a safe or favourable condition of the patient,” let me now take the other side of the question, and point out some of the exceptions to the rule that “rapidity of the circulation is an indication of danger.” And I may here observe, that a slow pulse more surely indicates a healthy state of things, than does a rapid pulse the reverse; in other words, the association of a slow pulse with puerperal disease is more rare than that of a rapid pulse with health, so numerous are the causes which tend to disturb the circulation at this time.

This division of the subject is much more extensive than the one I have just been engaged in considering, and the following remarks are to be regarded merely as a small contribution towards its elucidation.

Although the physiological state of the circulation during childbed is not one of excitement, nevertheless it is an eminently *excitable state*. That extraordinary susceptibility of the nervous system, which constitutes so prominent a feature of pregnancy, reaches its culminating point during this period, and furnishes at once a satisfactory explanation of many of the accelerated changes of the pulse, and teaches an important lesson in the hygienic and moral management of this class of patients.

We shall find it convenient to classify the cases of which I am about to speak under three heads, according as the pulse is *momentarily*, *temporarily*, or *permanently* quickened, a clas-



sification not only useful for purposes of arrangement, but of the highest practical value.

The causes which are capable of producing a *transient* excitement of the pulse in the puerperal patient are literally innumerable—a much more trivial thing acting as an *excitant* now, than would be sufficient at any other time. Any slight mental emotion or bodily disturbance will have this effect. Some altercation with the nurse, some muscular effort, or the entrance of the doctor into the room, are among the most common causes. Scarcely a day passes that I have not occasion to observe this momentary quickness of pulse produced among my hospital patients, by the simple circumstance of their asking for a pass to admit a visitor. In many newly-confined women, the act of nursing, even though unattended with any pain, and performed in a perfectly easy recumbent posture, is accompanied by a rise of ten or twelve beats in the pulse. This I have repeatedly noticed. It may be safely affirmed that an acceleration of the pulse, which is only of the transitory kind, cannot be of any consequence; and we can generally ascertain, within the limits of an ordinary visit, whether the hurry of the circulation be of the kind we are now considering or not. This I would call the natural or physiological irritability of pulse; but we must remember that there is such a thing as a *morbid irritability* of the circulation where the rise in frequency is disproportionate to the exciting cause. Here, caution and circumspection are necessary; for this condition of the pulse is apt to presage some form of puerperal disease.

The quickness of pulse does not always subside, however, so rapidly, but may continue for some hours. These cases are of more importance, and very justly occasion more uneasiness in the mind of the medical attendant. All such cases I would group together under the head of *temporary* acceleration of the pulse. Although this distinction may appear somewhat refined, yet it is not purely arbitrary; and there is this much also to be said in its favour, that the causes which operate in temporarily disturbing the circulation, are generally of a different kind from those in the class of cases last considered. Foremost among these causes is loss of rest, and the improper use of stimulants.

Nothing tends so powerfully as sleep to restore the equilibrium of the nervous and circulating systems after labour; and where a patient is deprived of her needful repose, the ill effects of this are exhibited in the quickened state of the pulse, the headache, and general nervous irritation.

Where the loss of rest is produced by any cause external to the patient,—such as a cross child, a snoring or coughing

nurse, or by noises in the house, &c.,—it should be removed, so far as is possible; but if its removal be impossible, a very good plan is to lessen the patient's acoustic sensibility, by putting wadding in her ears, a plan I have adopted with great advantage on many occasions. If the sleeplessness depends on the patient herself, we must only try and overcome it by opiates in some form.

A continuance of sleeplessness, with the arterial and nervous excitement consequent thereon, may be productive of serious ill-effects, mental or bodily, to the woman.

That the premature or too liberal employment of diffusible stimuli, in the shape of wine, brandy, or malt liquors, will occasion an over-excited state of the circulating system, is known to every one. If the physician have ordered any of these, he can very easily ascertain whether, or how far, the quickness of pulse is due to their influence. But when he is only cognizant of their effects, and in total ignorance of their administration, then indeed there is some excuse for his perplexity and alarm. Cases of this kind occur now-and-again from the clandestine use of the liquors in question; the patient, or patient and nurse, being to blame in the matter. Now, I cannot lay down any single rule whereby to diagnose this particular cause for excitement of the pulse. Of course, the practitioner would examine the patient with the strictest scrutiny, to see if disease be lurking in any part of the system. A negative result of this examination, together with the flushed countenance, the slightly excited manner, perhaps the odour of the breath, and, lastly, the time of the day when these symptoms are present, may lead to detection, or awaken suspicion. I remember attending a lady some years ago where this deception was very successfully practised upon me; and, no doubt, my daily anxious inquiries, and minute investigations to discover the hidden cause, afforded no little amusement to the patient and nurse, who, in this case, were in league together. It was not for months afterwards I found out the true explanation for the symptom which had so alarmed and perplexed me. Acting under the advice of the nurse, this lady was taking three or four glasses of port wine daily, in addition to some claret.

The nurse, however, is not always an accomplice in the imposition, and discovery in such cases is more easy.

We must not forget there are slight febrile attacks of the kind, commonly called "weid," which commence with a chill, and are frequently connected with some mammary irritation. These are attended with excitement of the circulation, which subsides in the course of 24 or 36 hours.

We come now to the third division of this subject, comprehending those cases in which the morbid frequency of the pulse is, by comparison, *permanent*, that is, remaining constant for some days, at least.

Although a continuing excitement of the pulse within the ten or twelve hours succeeding to delivery is always a symptom that calls for extreme caution and circumspection on the part of the attendant, yet it does not in every instance necessarily indicate the presence of organic disease, or of danger to the patient. In conformity with the plan of this paper, I shall endeavour to sketch very briefly some of the cases in which this symptom presents itself, *independently of any existing puerperal disease*.

When we meet with this excited state of the circulation, it is, no doubt, most satisfactory to find, upon a full and patient investigation, that it is a solitary symptom, unassociated with any other of a morbid kind; this, I say, relieves our mind of a great deal of apprehension. But then we naturally seek some cause for this vascular disturbance, and, till this be discovered, we are only half informed upon the case; and cannot with confidence assert that this symptom is not connected with organic lesion, nor can we venture to prescribe with any well-assured expectation of success.

Immoderate hemorrhage during or after labour may, as on any other occasion, be followed by an excitement or irritability of the pulse, persisting for many days, answering to Marshall Hall's description of "hemorrhage with excessive reaction." I rather think the puerperal condition to be peculiarly favourable to the production of this sequela of hemorrhage.

Of the puerperal patients who have had hemorrhage, all will not subsequently have a rapid pulse. It is much more likely to occur in some women than others; and I have observed that women of a pale, cachectic look, who lead sedentary lives, using little exercise, and rarely going into the open air, are peculiarly liable to this effect of hemorrhage, owing, no doubt, to the greater irritability of habit which this mode of life induces, and also to the deficiency of blood which is usually observed in these patients.

Extreme caution is needed before pronouncing, in any given case, that the frequency of the pulse is solely due to the hemorrhage; for flooding, as Denman has remarked, is a strong predisposing cause of puerperal fever, oftentimes of a low and very insidious form.

The inordinate use of tea for a length of time before labour, would seem to be capable of causing an increased rate of the

circulation. At least, I have now observed this connexion in many instances where nothing else could be assigned for the production of the symptom in question. These patients have generally been workwomen, accustomed to late hours, and with whom tea—often very strong tea—was an accompaniment of every meal, and constituted the staple article of their food.

I remember attending a lady (she was also seen by Dr. Hugh Carmichael), some sixteen or seventeen years ago, who was an exquisite example of this derangement. From day to day, after delivery, I was greatly alarmed to find her pulse ranging from 116 to 130. At length, one day, observing my portentous aspect, and guarded prognosis, she burst out a laughing. "Doctor," she said, "you are frightened at my pulse being so fast; but you needn't, for I can tell you the cause for it." Thereupon she acquainted me, that all through her pregnancy she indulged to an unlimited extent a craving she had for raw tea, which she always carried about with her, and used to eat dry out of her hand.

As I saw none of these patients prior to the setting in of labour, I cannot say what the pulse was then, but should suppose it was somewhat above the natural standard.

Where the act of nursing is productive of much suffering, whether from soreness or morbid sensibility of the nipples, the pulse will be considerably quickened by it; and if the child be frequently applied to the breast (under the mistaken notion of preventing mammary inflammation), time will not be given to allow the pulse to subside between one act of suckling and the next.

Patients labouring under organic disease of the heart form another group of cases where undue frequency of the pulse is occasionally observed after labour. I say "occasionally," for not every patient so affected shows this morbidly increased velocity of the circulation; nevertheless, I have now seen a good many instances of it.

When there exists valvular disease,—and nearly all these patients had signs of valvular disease, in some of its forms,—we can, I think, have little difficulty in understanding how any considerable change in the circulation must affect the action of the heart, in consequence of its power of adjustment or accommodation being so materially impaired. That the act of parturition makes a great change in the distribution of the blood, and general conditions of the circulation, is self-evident; and hence, I suppose, the great and undue excitement of the diseased organ. The incapability of the heart to make the re-

quired adaptation has proved a cause of death on some rare occasions.

The extraordinary influence exerted by the state of the mind over the organic functions of the childbed woman is one of the most striking and important features of the puerperal condition. The circulation participates largely in this influence; and accordingly it is not at all uncommon to find a quickened pulse in patients who are suffering under mental dejection, anxiety, or suspense, particularly if of recent origin. This is so well known, that it would be needless to enlarge upon it. Amongst patients at the hospital, a seemingly causeless quickness of pulse often suggests an inquiry into their social state or domestic circumstances, when we discover, for the first time, some hidden grief, or corroding care, which is the perpetual source of irritation.

An excited state of the pulse often precedes, for some days, an attack of puerperal mania; which is not at all surprising, inasmuch as mania itself is often brought about by mental inquietude of one kind or another. In patients of this description, if to quickness of pulse there be added sleeplessness, loss of appetite, and indifference to the child, we then have a combination of symptoms sufficient to justify the apprehension of a maniacal outbreak, and the adoption of such precautionary measures as prudence would dictate under these circumstances.

A very rapid pulse is on some occasions the first symptom—and for hours the only symptom—of the invasion of scarlatina, a disease most fatal to recently confined women.

In the winter of 1855-6, scarlatina was very prevalent in this city, and a number of the patients in the Lying-in Hospital had the disease after delivery. In several of them there was a very great and apparently unaccountable acceleration of the pulse for twelve, eighteen, or twenty-four hours before the occurrence of the eruption or of the soreness of the throat.

Dr. Churchill has recorded two very striking instances—one of them ending fatally—of extreme rapidity of pulse coming on within forty-eight hours after parturition, and apparently referable to latent scarlatina, certainly not to any puerperal disease, properly so called.

I shall now bring these cursory remarks to a close. I am very well aware that the subject is far from being exhausted, as I have only brought forward those cases which are of more common occurrence; whilst I have purposely omitted any account of the variations of the pulse induced by puerperal disease.

I will be excused for again urging the necessity of carefully

noting the state of the pulse at every visit to the puerperal patient. The importance of its indications can hardly be overestimated; and where it ranges above the healthy standard, this should be regarded as *prima facie* evidence that all is not right, and should lead to minute investigation; and even where no puerperal disease can be discovered to account for the symptom, a guarded prognosis should still be given, and cautious treatment adopted.

**ART. VIII.—Chronic Subacute Arachnitis.** By H. R. DE RICCI, M.D.

WE read in Cruveilhier's celebrated work on Human Pathology, the following interesting account:—"I shall ever reproach myself," says this distinguished writer, "for having committed the following serious mistake about a young school-boy aged eleven. He was of an idle disposition, and often pretended to be sick. When called in to see him, I found the aspect of his countenance natural, his pulse normal. He lay doubled up in his bed, without complaining of any pain, but was constantly asking to have stories read out to him. I ordered him up; and he then sat down near the fire, leaning against the sides of the chimney-piece. I made him walk before me, and he reeled like a drunken man. As he would not dress himself, his clothes had to be put on by force; and he even got some slight strokes of a cane; for every one was persuaded that the boy was only malingering. I was, however, very soon made aware of my serious mistake. In a short time the child died! died of subarachnoid meningitis!" This interesting account made a profound impression on me; so that I was ever after particularly cautious, whenever I met with cases where, without apparently any visible cause, the subject of my observations was, contrary to habit, either taciturn, morose, or sulky. I remember once being called to see a person who apparently seemed to be in rude health; her age was about forty-eight years. She did not complain of anything in particular, only said she felt ill. Her aspect was natural, her pulse good, her appetite excellent, and her rest unbroken; but still she said she was ill. On the third day of my attendance, she told me she *thought* she had a pain in her head; and on my asking her to walk across the room, she stood up and put out a hand, as if to hold on by something, or as a person would do walking in the dark. On the fourth day it seemed to me as if her answers, though correct, were slower; and on the following

day, having had the opportunity of speaking to an old friend of hers, I obtained such information as confirmed me in the diagnosis I was gradually arriving at in my own mind. Up to this time my treatment had been of the simplest kind. Having been informed that the catamenia had ceased for about six months, I had ordered six leeches to the perineum, and some mild aperient medicine; but when I now was told that for some time previously the patient had been losing her memory, and that so long as six weeks before she could not at times remember in the evening where she had been to visit in the day, I determined on adopting a more decided treatment, and applied at once twelve leeches more to the perineum, and a blister to the back of the neck. The following day, the fifth, the patient was rather worse; her pulse, though of normal strength, beat only 46 in the minute. When asked how she was, she took some time to consider, and then answered, "Very well;" but I remarked that when she smiled her mouth was slightly drawn to the left side; and occasionally a sudden change—a cloud—would pass across her countenance, and she would carry her hand to her head, laying it on a spot corresponding to about the middle of the left parietal bone. When asked what ailed her at those moments, she would tardily answer, "I do not know;" when it would be suggested to her if it was a pain, she would answer, "Yes." There was no difference in the power of grasping of the two hands; nor did the mouth show any deviation, except in smiling. All these symptoms, so trivial apparently to the uninitiated, filled me with alarm, greatly to the surprise of the patient's friends, who still saw nothing in her to make them apprehensive. I determined, however, if possible, to be in time; and having fully made up my mind that it was an insidious case of subacute arachnitis of the left hemisphere, commenced giving her small doses of calomel at short intervals. On the following day, the sixth of my attendance, all the symptoms were aggravated; the pulse was slower, the mouth more crooked; the heaviness had now become stupor, which, by the following day, passed into absolute coma, accompanied by the unconscious discharge of both rectum and bladder. This was now the seventh day of my attendance; the case had been steadily progressing from bad to worse; and the friends at last, having become fully alive to the correctness of my views, acknowledged with alarm that I had not overrated their relative's danger. What was to be done? The mercury had not as yet produced its specific effect; if such could be obtained soon enough, the patient might yet have a chance, though indeed only a remote one. I im-

mediately ordered the blistered surface on the back of the neck to be dressed with mercurial ointment, and a suppository containing fifteen grains of strong mercurial ointment to be introduced into the rectum. In less than twelve hours she was ptyalized, mainly owing, I think, to the employment of the mercurial suppository, which, after many years of trial, I never knew to fail, whenever I wanted rapidly to produce the specific effect of the metal. At the same time that I adopted this internal treatment, I ordered blisters to the tops of the arms, and sinapisms to the legs and feet, to act as derivatives, and stay, if possible, the mischief going on on the surface of the brain, until the mercury taken up in the circulation could produce its specific effects.

The mischief was stayed, and bravely did the mercurial do its duty; I never saw so clear and satisfactory a proof of cause and effect as in this instance. We had leeches, blistered, and purged; we had applied sinapisms to the legs and feet; but still the case progressed, and the stupor merged into profound coma. Nothing seemed to be of any avail in stopping the onward course of this insidious disease, until the fetor of the breath first, and then the characteristic marks upon the gums, gave unmistakeable evidence of the mercurial action being established; then, and not till then, did the patient rise from that stupor; she opened her eyes, spoke clearly and sensibly, passed no longer under her, and her mouth returned to its normal site. Her pulse, which had been so low, now rose to 70, and soon after to 75,—principally owing, I think, to the irritation caused by the severe ptyalism under which she now laboured. The disease was evidently checked, and every one looked forward with hope to a speedy and favourable termination. The salivation being very abundant, all mercurial applications were stopped; and as the soreness of her mouth prevented her from taking a sufficient quantity of food by that channel, I ordered her nutritive injections, containing each an egg, two ounces of beef tea, one ounce of wine, and three grains of quinia, to be administered every third hour. The blisters on the back of the neck and over the arms were, at the same time, kept in activity by dressing with savin ointment, as I conceived that the continued discharge from them would help to complete what appeared to be the commencement of a cure. Her mouth was, at the same time, freely washed with a weak solution of nitrate of silver, by which means it soon became sufficiently healed to permit her to take food freely. We had now arrived at the fifteenth day of my attendance—I do not say of her illness, as that had evidently been going on for



many weeks before I was called in. On the evening of the fifteenth day, she appeared somewhat heavy and sleepy; she also moaned occasionally, and again her hand went slowly towards the same spot, over the left parietal region. With some difficulty I succeeded in rousing her; and having found that the blisters over the arms were nearly dried up, I applied at once fresh blisters over the same spots, and administered two drops of croton oil, which in a few hours brought away several copious dark-coloured and highly fetid stools, by which she seemed greatly relieved; and on the following morning, at my first visit, I found her sitting up in the bed, eating her breakfast with the greatest relish, without pain, stupidity, or drowsiness. On dressing the fresh blisters, I found a copious discharge; to it, therefore, no doubt, aided by the active purgation, was due the well-marked improvement from the previous evening. Having duly considered this in my mind, I came to the conclusion that the treatment most likely to insure success would be first to establish a permanent purulent drain in the vicinity of the head, and then to continue, if possible, the action of the mercury a little longer, and to follow it up by a course of iodide of potassium, at the same time that the patient was amply supported with nourishment and wine. I therefore told the patient's friends that I considered it necessary to establish two issues, one 'in the top of each arm, to endeavour to prevent, if possible, the recurrence of a relapse such as had alarmed us so much a few days before, on the drying up of the blistered surfaces. They seemed filled with astonishment at my announcement; they fully believed that the worst was over; that the patient was on the high road to recovery; that I was only an alarmist; and ended by absolutely refusing to permit anything which would constitute, in their eyes, a permanent blemish. I had not been prepared for such a refusal; and thinking that their own good sense would after a few hours prompt them to revoke it, I dropped the subject until the eighteenth day, when I again strongly urged it on them; and on their repeated refusal, I asked for a consultation. The gentleman who was selected to meet me took a most unfavourable view of the whole case, which he regarded as hopeless, although the patient looked better and was livelier than she had been yet since her illness. He agreed with me that the necessity for a permanent drain was clearly pointed out; but he differed in the locality. He recommended the top of the head, in preference to the top of the arms; and deprecating strongly the use of caustic potash, insisted on the issue being established with a blister of two

inches diameter, to be kept open by Albespeyre's paper. I argued strongly for my plan, but in vain; for I knew well the divers idiosyncrasies of my patient, and I knew that she would do anything rather than allow any portion of her head to be shaved. I also felt assured that any cause of annoyance to her should be studiously avoided at this moment, as it would tend seriously to complicate an already too serious disease. I also felt that a blister would never establish the drain I required, at the same time that the locality selected was full of other objections also. However, I argued in vain; with the dull tenacity of age, he persisted in his views, and left the house. As I had anticipated, the lady most decidedly set her face against any of her hair being removed; she insisted that she was better, that she required nothing, and begged to be left alone. This day she had been much better than ever I had seen her during her entire illness; and so I left her in the hands of her friends, who all undertook to persuade her to let the scissors to her head. In about four hours I was sent for, to say that she had at last consented to have her head shaved; but that she was rather weak. I went to see her at once, but was shocked at noticing the change that had come on her since the morning; the vacant stare, the flushed cheek, the crooked mouth, the pulse of 130; all told clearly of the mischief that had been done. However, the hair was removed and the blister applied. It never did any good, while the preparation for it did such incalculable harm. On the following morning this poor lady was completely comatose; her pulse had fallen to 96; the buccinators flapped with each expiration, and the rectum and bladder were discharged without her knowledge—fearful contrast with the hopeful condition of the preceding day. Choice would now have suggested to me to abandon the case—duty called on me to remain; I did so, and at once set to work to endeavour, if possible, to allay the irritation that had been freshly lit up in a previously diseased brain by that mental torture which had been inflicted on her for four hours on the previous day. Her stomach was too irritable to take food, and she had some hiccup. I had, therefore, to rest satisfied with ordering her some iced brandy and soda-water, and to return to the nutritive injections. On the following day her pulse was lower, and her face less flushed; but she still lay on her back, unconscious of all that passed around her. The same treatment was continued, with the addition of mercurial dressings to the blisters, which still were discharging. The next morning a fresh symptom had set in; her right arm was stiff; and when I attempted to straighten it, the tendon of the biceps

at the bend of the elbow felt like a cord under the touch. This clearly indicated increasing mischief, and determined me on again making a bold push with the mercury as the only chance, though but a forlorn one, of restoring her to health, at the same time that I established two issues with *potassa fusa* on the top of each arm. Fearful, however, of making her mouth too sore, and thus stopping up the main ingress to nutrition, I proceeded cautiously with inunctions until the faintest fetor could be discovered from her mouth, when I had her immediately cleansed from all mercurial dressings. The rigidity of the left arm, which for the last three days had gone on increasing, now began to give way; and on the twelfth day from the return of the coma, and the thirty-third of my attendance, it was completely relaxed, and some power was returning in the fingers; the issues, also, were doing well. She still, however, remained in a state of partial stupor; her eyes would occasionally open, but she never spoke; and her stools passed away unconsciously. There was, however, a certain degree of improvement; her breathing was no longer stertorous; she had no hiccup; and she took her food well. Her bowels were torpid; but they could always be roused with small doses of croton oil. She remained in this way till the eighteenth day since she fell into her last state of coma, when, the issues I had put into her arms on the seventh day being in an active state of suppuration, and aided no doubt by the mercury, which had now produced a decided though gentle action on the gums, she opened her eyes and spoke for the first time since the day she became comatose, sixteen days before. On the following day I found her better, and the improvement continued daily, but with one exception—she still passed under her. The power of grasping was equal in both hands; her mouth was perfectly straight, except when smiling; she complained of no pain; eat well, drank well, and slept well; but still the rectum and bladder were discharged unconsciously. Thus she continued for many weeks daily improving, except in that one particular, till on the seventieth day of my attendance she was attacked by what she herself called a *spasm*; she could not describe it, though she was perfectly conscious. It was not a pain anywhere, she said, but it was *all over her*. No doubt, it was some form of epileptoid seizure. On the following day these spasms became more frequent: the day after I remarked that the issues, which for a few days previous had been yielding *less pus* than usual, had become *nearly dry*. I dressed them with cantharides ointment, but to no purpose. On the following day, the right arm became again rigid; the spasms

increased in frequency; the patient became drowsy; she refused her food; and finally, effusion having taken place into the bronchial tubes, she was suffocated on the 111th day of my attendance.

This case, which I have given at such length, was, I think, most interesting, and to me full of instruction. I believe that if I had seen the patient a fortnight sooner, I might have had a different issue to recount. I think that even under the circumstances I attended it, if on the eighteenth day I had been permitted to insert the issues I so strongly urged, she might yet have had a chance of doing well; but that four hours' mental strife was fatal to her recovery; and her brain, already seriously affected, never recovered from the worrying it got on that unlucky day.

I may, perhaps, be asked why I publish a case which ended unsuccessfully. Well, because I believe that we should recount not only our successful cases, but also our failures; for often these last will teach us even better than the former. I knew the power of mercury and the use of issues; but I never saw their value, especially of these last, so clearly exemplified as in this case: they were perfect indicators of the patient's state. When freely discharging, the patient did well; when not discharging, the patient was ill; and when they actually dried up, the last scene closed upon our labours. In the next number of this Journal I shall contrast this case with one of acute arachnitis, which also terminated fatally; but in which I had the advantage of a *post-mortem* examination, which I regret extremely that I could not obtain in this instance.

ART. IX.—*Plastic Operations on the Female Genito-Urinary Organs*\*. By THOMAS. E. BEATTY, M. D., M. R. I. A., F. R. C. S.; Honorary Fellow of the King and Queen's College of Physicians; Ex-Professor of Midwifery, Royal College of Surgeons in Ireland; Physician to the City of Dublin Hospital; President of the Obstetrical Society; Ex-President of the Pathological Society; Corresponding Member of the Obstetrical Society of Edinburgh, &c., &c.

THE subject of Plastic operations with metallic sutures has occupied so much attention during the last few years, that I am induced to hope the following cases may not be uninteresting. There is no doubt that the revival of the use of metallic su-

\* Read before the Surgical Society, March 9, 1861.

tures by Marion Sims was a very great boon to surgery ; and although we may not be disposed to go the whole length with him, when he states\*, " I declare it as my honest and heartfelt conviction that the use of silver as a suture is the great surgical achievement of the nineteenth century," we must acknowledge that to him and Dr. Bozeman we are indebted for vast improvements in the treatment of cases that formerly were considered hopeless.

Although, as Dr. Simpson has shown, these very eminent operators have been forestalled by Mr. Gossett, who, in " *The Lancet* " of November 29th, 1834, describes a case of vesico-vaginal fistula cured by him by means of " gilt-wire suture," and the steps of whose operation bear a very striking resemblance to that of the American surgeons, we must still award the merit of working out and perfecting the process to the latter. It is just as the credit of the discovery of the Atomic theory in chemistry is awarded to Dalton, because he laboured hard, and with great skill and patience completed that theory ; which did not originate with him, but was notoriously first promulgated by Higgins, then Professor of Chemistry in the Royal Dublin Society. In noticing the operations for vesico-vaginal fistula, we should not forget that to Mr M. Collis, of this city, we are indebted for the first impetus given to these operations in this country.

His paper on the subject, read before the Obstetrical Society in May, 1856, and published in the " *Dublin Quarterly Journal*," details an operation differing from those that preceded it, in the proposal to split the edge of the fistula all round, instead of cutting off a portion of it ; and then bringing the split edges together by means of quill-sutures. The first operation done by Mr. Collis in this manner, he was kind enough to submit to my inspection, and I found the orifice quite closed. I am inclined to think that this proposal of Mr. Collis to split the edge, instead of paring away tissue, might and ought to be adopted in cases where the opening is very large, and saving of substance is of importance. In most cases the tissue is sufficiently thick to afford of this being easily done ; and when it is, there is less strain on the ligatures, of whatever material composed.

It will be seen that, in the following cases, some of the operations differed from that proposed by either Mr. Sims or Bozeman, and partook more of the modifications suggested and practised by Dr. Simpson, to whose lecture, illustrated by wood-

\* Anniversary Discourse, New York, 1858.

cuts representing the different instruments employed, we are all so much indebted for accurate and useful directions. In one of them, the operation contrived by Dr. Battey, of Georgia, U. S., was for the first, and I believe the only time, performed in this country.

CASE I.—M. R. was sent up to me by Dr. Mackesy from Waterford, in the hope that some relief might be obtained from her present lamentable condition. She is a nice interesting person, twenty-six years of age, and two years married. She was confined on the 18th of January, 1859, of her first child. The labour was very severe, lasted twenty-four hours, and was terminated by the assistance of the forceps. During the operation the perineum gave way, and the rent extended through the spincter ani. She was confined to bed for three weeks, and recovered slowly. It is now five weeks since her delivery, and she complains that she cannot retain the contents of the bowels when at all liquid. If they are solid, she can pass them voluntarily but otherwise they escape without her knowledge. Flatus is, in like manner, unconsciously passed. She is still nursing her infant. She entered the private ward of the City of Dublin Hospital, under my care. To ascertain the extent of the laceration, I placed her on her back on a high table, with the legs bent, and the feet resting on the table in the lithotomy position; I then introduced M. Sims' duck-billed speculum into the vagina; and having it well held up towards the pubes, I got a full view of the back of the vagina and the torn perineum. I now found that, in addition to the perineum and anus, the rectum was split up for about an inch and a-half. The edges of the rent were quite healed; and the corners of the lower part, near the verge of the anus, were rounded off, just like the corners of a hare-lip. From the nature and extent of this injury, it was plain that two operations must be performed; the first with a view to close the rent in the rectum and sphincter ani, and the second, at a future time to restore the perineum. As it was of great importance to have the bowels in a quiescent state during the progress of the cure, after the operation, I had them well emptied by medicine, and the rectum well washed out by injections, for three days previously; and the day before the operation, a quarter of a grain of opium was given every fourth hour, to secure torpidity of the intestinal canal. Having made these preparations, I proceeded to operate on the 18th of March, 1859, in presence of Mr. Tufnell, Dr. Churchill, and Mr. Croly, house-surgeon to the hospital. The patient was placed on her

back, as already described, on a high table near a window, with a good light shining into the vagina, now well displayed by the speculum already mentioned, which was well held up by Mr. Croly. The first step in the operation was to pare the edges of the rent in the rectum. This was effected by sticking a hook into the lowest point of the torn sphincter at one side, and with a knife cutting off the edge from the bifurcation above down to the point, just as in the operation for hare-lip. This was repeated on the opposite side, leaving a  $\Delta$ -shaped cut into the recto-vaginal septum an inch and a-half long. The edges were now brought together by means of iron-wire sutures, passed in the following manner:—A good-sized curved needle, holding in its eye a piece of iron-wire about six inches long, was firmly grasped at a right angle, near to the eye, by a strong *porte aiguille*, which was kept tightly closed on the needle by a slide run up on the blades. The point of the needle was then struck into the lining membrane of the vagina, close to the upper angle of the wound, on its left side, and at a distance of a quarter of an inch from the cut edge, and driven through the tissue between the rectum and vagina until the point appeared through the cut edge. The needle was urged across the gaping wound until it was made to enter the opposite cut edge at a point corresponding to that from which it emerged in the other; and being forced through the submucous tissue as before, the point was brought out at a quarter of an inch from the edge. When sufficient of the needle was passed through, it was caught by a strong forceps, and the slide on the *porte aiguille* being retracted, the needle was cast loose on the side where it had first entered, and was pulled out on the right side of the cut, carrying the iron-wire with it. This was now cut off with a pair of scissors near the eye of the needle, and so the first ligature was passed. In a similar way four other iron ligatures were made to traverse the gap in the septum, the last being through the edge of the anus. A thin leaden plate, an inch and a half long, and half an inch broad, with five holes pierced down the centre, was now prepared; and through the holes the ends of the wires were passed, beginning at the upper end, and so on to the last. The shield was then pressed down to the wound, while the ends of the ligatures were held on stretch in the left hand. This had the effect of drawing the cut edges into close apposition; and to secure them in that position, perforated shot were slid down over the ends of each pair of wires; and the shot, seized in a strong pair of forceps, was firmly forced down on the shield; and while the wires were drawn tight, the shot was strongly compressed, so as to hold

the wires securely fixed. This having been done for the five sutures, the ends of the wire were cut off near to the shot, and the ends of the cut wires were bent down on the shield. The operation being thus accomplished, the patient was put into bed, and a quarter of a grain of opium was given every third hour. Directions were given to have the bladder emptied by the catheter three or four times in twenty-four hours, so as to prevent the possibility of the urine coming in contact with the wound. This was strictly attended to by Mr. Croly, who watched the case with the most laudable care. No unfavourable symptom occurred until the 24th, the sixth day after the operation, when a pretty smart hemorrhage took place from the vagina, which lasted only a quarter of an hour, and was stopped by injecting cold water into the passage. On the 26th, the eighth day, the ligatures were removed, in the following manner:—The patient was placed in the same position as on the day of the operation; and the same speculum having been introduced into the vagina, the shield and lead buttons of shot were exposed. With a long pair of sharp-pointed scissors the wires were successively cut across, close to the shield, beginning at the outer one; and all the shot being thus removed the shield came away, leaving a perfectly healed cicatrix, with a surface and edges as smooth as if it had been pressed with a hot iron. An injection of warm water was now thrown into the rectum to soften the fæces, and the bowels were slowly relieved without any laceration of the newly-united parts. This constituted the first part of the operation. It was most successful in its results. The patient regained complete control over the bowels, and nothing escaped without her consent. She remained in hospital for ten days afterwards, when I advised her removal to the country for a short time, previous to any further operative proceeding. On the 27th of April, she returned; and on examination, I found the parts perfectly sound and solid; and so much contraction had taken place, that the gap in the perineum did not appear nearly so large as formerly, and it was quite manifest that a smaller amount of closure would be necessary than was expected. On the 2nd of May, I proceeded to finish the operation. This was done by paring about three-quarters of an inch of the edges of the torn perineum from the anus forwards, and then uniting the cut edges by means of three iron-wire sutures without a button. The ends of the wires were simply twisted together, and cut off about a quarter of an inch from the wound. The same precaution with respect to the urine was observed, as in the former operation. It was drawn off with the catheter every



six hours, and the bowels were kept quiet by opium. On the eighth day the sutures were removed, by clipping one side of the noose with a fine-pointed scissors, and drawing the wire through. The wound was found to be perfectly healed; and the patient left the hospital on the 6th of June, a much happier woman than she had entered it.

CASE II.—On the 28th of May, 1859, I was requested by Mr. Banon to see Mrs. H., who had come up to town from Limerick for advice, in consequence of incontinence of urine since her confinement. This had taken place on the 3rd of the month. Her labour was of her first child, twenty-four hours in duration, and very severe. No instruments were used in the delivery. She suffered a great deal of soreness afterwards; and on the sixth day the urine began to come from the vagina, and had continued to do so ever since. On examination, we found a large opening into the bladder from the vagina, of a size sufficient to permit a walnut to pass through; the edges of the opening were thick and fleshy, and in some places granular; the whole mucous membrane of the vagina was inflamed and raw. I expressed an opinion that it was a very favourable case for operation, and at Mr. Banon's desire I undertook the case. On the 31st of the month I proceeded to operate, in presence of Mr. Banon, Dr. Churchill, and Mr. M. Collis. The patient was placed leaning over the edge of a low bed, with her chest supported by pillows, and the buttocks turned towards a window, through which a good light came. The duck-billed speculum of Sims was introduced into the vagina; and being well held up, the aperture in the vagina was brought well into view. The edges were well pared all round. Considerable bleeding took place from the very vascular tissues; this was arrested by tincture of matico; and then five stitches were put in by means of the tubular needle, made by Mr. Young, the eminent cutler in Edinburgh. - This needle was first described by Dr. Simpson, and it is figured in his lecture on vesico-vaginal fistula, in the "*Medical Times and Gazette*" for January, 1859. It is a most perfect instrument, does its work in superior style, and is easily managed. In the operation for vesico-vaginal fistula, the wound being made to close in a transverse direction, the stitches are introduced from before backwards, as the patient lies before us, and the tubular needle enables us to do that with the greatest ease. The wire, cut to the length required, is introduced into the tube, and pushed forward, until the end of the wire appears at the point of the needle; it is then withdrawn, until the end just

disappears within the tube; and having dipped the needle in oil, the point is driven into the membrane lining the vagina, half an inch from the cut surface; and being passed between the bladder and vagina, taking care not to penetrate the former, it is carried out through the raw edge, and then being pushed on, it is made to pierce the far side of the freshly-prepared border of the aperture; and being passed as before between the vagina and bladder, it is made to emerge through the wall of the vagina, at half an inch of the other side of the opening. When the point of the needle is seen well above the soft parts, the wire is steadily pushed forwards in the tube, and emerges from the point of the needle. The extremity of the wire is then seized with a long forceps, and pulled well downwards, while the needle is withdrawn over the wire, which it leaves thus safely lodged in its proper position. In this case five stitches were found to be necessary. They were fastened over a leaden shield, or button, differing from Boze-man's in this particular, that while his button has but one row of holes down the middle, through which the wires are brought (two through each hole), the button I used had two rows of holes parallel to each other, or rather five pairs of holes, instead of five single holes, as originally described by Dr. Simpson in the lecture already referred to. The object of this was to get rid of the perforated shot as a means of closing the stitches, and to close the wound by twisting the wires after they were brought through the double holes. The twister originally designed by Dr. Coghill, consisting of an iron rod five inches long, with two very short tubes a quarter of an inch long, attached on either side of its extremity through which the wires were passed, served to secure the stitches. The wires, being passed through the tubes, or rather holes, in the bulb at the extremity of the rod, were held firmly in the left hand; and the instrument, pushed down to the leaden button, was twisted three or four times, making a close and regular cord of the wires. It was gradually withdrawn as the twisting motion was given, and finally withdrawn over the wires; these were cut off within a quarter of an inch of the plate, and the other pairs of wires were treated in the same way, until the whole were secured; the cut ends were then folded down over the plate; a short gum-elastic catheter, with an Indian rubber bag attached to it, was secured in the urethra; the bag had a stop-cock at its other free side, to allow of its being emptied. The patient was placed in bed, lying on her face; a grain of opium was given, and she was ordered a quarter of a grain to be taken every third hour. A good nurse was put

in charge, and Mr. Banon and myself visited her every morning and evening. On our visit the second morning we were startled by hearing that the catheter had slipped out in the night, and that she had passed water by the urethra; no urine came through the vagina. The instrument was now firmly secured, and was removed every day, and washed. No untoward occurrence took place afterwards. On the eighth day I removed the stitches, by placing her in the position as on the day of operation, and displaying the apparatus by means of the same speculum. The stitches were removed by cutting one side of the loop with a sharp-pointed scissors, and drawing out the wire by a forceps. On the removal of the plate, we were gratified to find the whole wound perfectly healed, the cicatrix presenting a uniform smooth surface, as if it had been pressed with a hot smoothing-iron. The catheter was kept in for two days more, the opium was stopped, and the bowels were freed. After this she was allowed to get up; the urine came naturally by the urethra, and in a few days more she returned home. I have heard lately that this patient has been since safely delivered of a living child, and that she suffered no inconvenience of any kind during or subsequent to her labour.

CASE III.—Mary Nolan, aged twenty-three years, after first labour, which was very long and difficult, suffered great soreness of the vagina for some days, and then found a sudden burst of urine through that passage, which has continued to come in that way ever since. Her thighs and buttocks are excoriated. On examination in the position already described, a very frightful destruction of parts was discovered; in fact, the whole front of the vagina was gone, and one looked through the chasm into the cavity of the bladder. The gap extended from the vesical end of the urethra up to the cervix uteri, and to an equal distance transversely. It was nearly square, and measured two inches in every direction. The patient suffered severely from inversion of the bladder, which constantly took place when she stood up or walked, hence she was obliged to preserve the recumbent posture. Notwithstanding the unpromising nature of this case, I was unwilling to allow the sufferer to lose the chance of some relief, and I determined to make an effort to close this formidable breach. It happened that Dr. Battey, of Georgia, U. S., was in Dublin just at this time. He had designed and exhibited to me a modification of the operation of Sims and Bozeman, particularly calculated for this case, and I requested him to visit the patient with me in the hospital.

We agreed to try his operation, which I performed a few days after, in presence of Mr. Pirrie, the eminent Professor of Aberdeen, and author of the great work on surgery, who was on a professional visit to this city at that time, along with my colleagues in the hospital, and some other friends. The earlier steps of the operation were the same as those described; the paring of the edges of the opening, and the passing of the wires were the same. The great extent of the aperture made it necessary to place nine sutures *in situ*. All this was done, and now came the difficulty of closing such an enormous gap; and at this stage Dr. Battey's contrivance came to my aid. A bar of thin lead, two inches long, and one-eighth of an inch wide, perforated with nine holes to correspond with the nine sutures, was prepared; and instead of the second row of holes, as used in the plate in the last operation, nine notches were made in the edge of the plate, corresponding with the nine holes. Through each of the holes was now passed one of the distal ends of the wires, and then perforated shot was run down upon each of the wires, and closed on it by compression with a strong forceps. When all the nine wires were thus secured, the proximal ends which hung out through the anterior edge of the opening were grasped in the left hand and pulled strongly downwards, thus drawing the leaden bar into close contact with the distal side of the opening. The traction was continued and increased, and by degrees the upper edge was made to approximate the lower. This occupied a considerable time, and was much assisted by placing the end of a thin flat piece of wood, like a flat ruler, under the proximal wires, and pressing the end firmly upwards against the part perforated by the wires, while the leaden bar and the parts against which it was lodged were drawn down. By this means the raw edges were finally made to touch. The next point was to secure them in that position. This was done by turning up each wire in succession, and lodging it in the notch in the edge of the bar above described; and when safely lodged there, the edge of the bar was strongly compressed behind it by the point of a strong forceps, thus fixing the wire securely in its place, the distal end already secured by the perforated shot, the proximal end now firmly fastened in the corresponding notch; in this way the nine sutures were fastened. The ends of the wires were now, for further security, twisted by the twister. Some idea of the great difficulty of this operation may be formed from the fact, that three hours and a-half were occupied in its performance. The catheter was introduced and secured as usual, and opium was given as already described. My expectation of success, small

at first, was diminished on the fourth day, when urine was found trickling from the vagina. At the end of eight days the apparatus was removed; the wound was found apparently closed on the right side for a short way; but the strain had been too great on the left, and the wires had cut through the soft parts, leaving a large part ununited. I confess I was not disappointed at the failure in this operation. It was almost impossible to hope for a closure in such an enormous gap at the first trial. The patient remained for some time in hospital, and then went home to the country. She promised to return; and I intend, on some future occasion, to try another operation.

Prolapse of the uterus, when complete, is well known to cause very great inconvenience to the sufferer, and a variety of means have been proposed for its permanent relief; excision of portions of the mucous membrane; destruction of parts of the prolapsed surface by the application of strong nitric acid, &c., have been had recourse to. The latter has been followed by great success in many cases in which I have employed it; but the cure is spread over a very long time, owing to the number of applications that are necessary, and patients get tired of waiting through the long process and the numerous operations. The pessary is only a palliative, and requires looking after and arranging from time to time. The most effectual and speedy remedy for this displacement is that proposed by Mr. Baker Brown, consisting in a permanent closure of the vulva. Two cases were thus treated in the City of Dublin Hospital by me, with complete success.

CASE IV.—Mary Kelly, aged sixty-five years, was admitted on account of a very large prolapse of the uterus, with which she had been afflicted for many years. She was the mother of several children, and the prolapse commenced after the birth of the last child, twenty years ago. It had lately increased very much. It was permanently down, not returning when she assumed the horizontal position. The surface was dry, and covered with cuticle, except near the lower part, where some spots of ulceration existed, owing to the trickling of urine over it, and the friction to which it was subjected in walking. As she was a widow, past child-bearing, I considered this a suitable case for the operation of closing the vulva. The woman was kept in bed for some days; the prolapsed parts were returned within the vagina, for the purpose of accustoming them to their natural, but now unusual, position. The operation was performed in the following man-

ner:—The woman was placed on a high table in the lithotomy position, and securely held there close to a window, through which light fell freely on the vulva. I made an incision in the mucous membrane of the labia majora, near the line where it joins the common integument. This incision commenced on the right side, nearly on a line with the orifice of the urethra, was carried all down that side to the fourchette, and up the opposite side to a point corresponding with that from which the incision started. A similar incision, parallel to the last, was made all round at one-fourth of an inch within the vagina. When this was completed, the strip of mucous membrane between the two incisions was carefully dissected off, leaving a raw surface, one-fourth of an inch broad, all round the vulva. Double iron-wires were now passed from side to side by means of needles driven through the common integument, one-fourth of an inch from the cut edge, then through the middle of the raw surface, then across the vulva and through the opposite raw surface, and so out through the integument at a distance from the cut edge similar to that where it had entered. Four of these double wires were thus passed through, and the wound was closed by laying a piece of bougie of proper length along the right side, first between the wires as they emerged from the skin, and making four loops round it by twisting the ends of the four pairs of wires together, and a similar piece of bougie was laid along the left side between the wires; and the edges of the wound being strongly drawn together by pulling the wires, they were twisted together on that side also. A firm quilled suture was thus effected. In order to make the junction of the edges of the integument as complete as possible, three fine iron-wire stitches were made through the skin in the intervals between the deeper sutures. Opium was ordered, as in the former cases; and in order to prevent the contact of urine with the cut edges, the catheter was passed every six hours. On the eighth day I removed the sutures, and was gratified to find the vulva closed by a firm cicatrix, leaving a small aperture at the upper part corresponding to the orifice of the urethra.

CASE V.—R. Byrne, aged 60, was admitted in the City of Dublin Hospital, with a very large prolapse of the uterus, which had been down for many years. After preparing her for treatment as had been done in the former case, the operation, as just described, was performed. The same after-treatment was pursued; and, at the end of eight days, a similar happy result was found.

This is an operation that, I think, is only applicable to old

women. I am aware that Mr. Baker Brown proposes to treat younger and married women by an operation similar to that just described, but of less extent,—closing, in fact, only a portion (the posterior) of the vulva. I am afraid such a partial closure would not prevent the escape of enclosed prolapsed viscera; but that bit by bit, and by degrees, the bulk above would insinuate itself into the aperture, and finally escape from the pelvis, as before.

*Case VI.*—In October, 1860, I was requested by Mr. Banon to visit a patient under his care in Jervis-street Hospital. I found her labouring under the distress caused by vesico-vaginal fistula; and thinking it a very favourable case for operation, I placed myself and my instruments at his disposal whenever he thought fit to operate. This he did on the 15th of the month; and he has lately favoured me with the following letter respecting the case:—

*“Mountjoy-square, February 21, 1861.*

“MY DEAR DOCTOR,—I send you the particulars of the case of Catherine Ormond, on whom I operated on the 15th October, 1860, for vesico-vaginal fistula, assisted by you; and request you will have the kindness to read them, when bringing the subject forward at the Surgical Society. I take this opportunity of telling you that I had a few days ago a letter from Dr. Riordan, of Bruff, informing me that our former patient, Mrs. Hayes\*, on whom you so successfully operated on the 31st of May, 1859, has recently been safely delivered of a full-grown child, without any injury whatever to the cicatrix of the fistula.

“I am very truly yours,

“A. BANON.

“Catherine Ormond, aged 19, was sent up to me by Dr. Seward, of Caherconlish, and admitted to Jervis-street Hospital, on the 29th of September, 1860. She states, that three months previously she was delivered of her first child, after a very severe and tedious labour, lasting three days; but she made apparently a good recovery, and found nothing wrong in passing water for nearly four weeks subsequently, when she lost all power of retaining it; and it continued to trickle through the vagina ever since, keeping her in a constant state of irritation and misery. On examination, an oval fissure nearly an inch in length, extending obliquely from left to right, was seen occupying the vesico-vaginal septum at its lower part, and having a portion of the mucous membrane of

\* This was the patient of case No. 2.

the bladder protruding through it. On the 15th of October, the bowels having been previously emptied by a purgative enema, I proceeded to operate, assisted by Dr. Beatty, and in presence of my colleagues in the Hospital. First paring the edges, which I succeeded in doing by removing a complete ring of the circumference of the fistula without a break, five iron-wire sutures were introduced, and secured, in the manner described by Dr. Beatty, on the leaden plate. The patient was kept lying on her face for ten days. A No. 10 male catheter, fixed in the bladder by tapes, and daily changed, conveyed the urine by drops into a vessel suitably placed, so that no accumulation of this fluid could take place in the bladder. Occasional doses of opium were given, and the bowels fortunately did not act during the whole period. The vagina was daily syringed with tepid water. On the eighth day, the sutures and plate were removed, when the fistula presented a smooth appearance, and looked quite healed. On the tenth day, the catheter was removed, and the woman was allowed to get up. From this time she experienced no inconvenience whatever; and left the Hospital shortly afterwards, quite well, the fistula presenting an appearance of firm union. She was recommended separation from her husband for some months. She has recently written to me, expressing her gratitude for her cure."

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ART. X.—*Cases in Surgery*. By EDWARD HAMILTON, M.D., T.C.D., F.R.C.S.I., one of the Surgeons to Steevens' Hospital, and Lecturer on Physiology in its Medical School.

*Double Hare-Lip, which had been submitted to two previous operations without success.*

JAMES BENTLEY, aged three years, a very fine little fellow, but most wayward and uncontrollable, presented the deformity of double hare-lip in a marked degree; the centre piece extending about half the depth of the fissure. On the left side the cleft was prolonged through the palate; the right alveolar arch was prominent, one of the incisor teeth projecting somewhat into the gap. The segments of the lip were adherent to the gum; the edges did not convey to the touch the usual soft feel, so characteristic of the red border of the lip. The mother states that he had been twice operated on for this deformity: first, at the age of six months, when, according to her account, in two days after the operation, "the needles burst out," leaving



the fissure ununited. He remained in this state until he was eighteen months old, when the operation was again undertaken by another practitioner. She states, that after two days, the needles having been removed, the wound bled freely, and its lips separated,—thus rendering this operation also nugatory. With this state of things, and the history of the case, coupled with the great terror and violent struggles of the little patient, resulting from his painful experience of two previous operations, I felt grave misgivings as to the result, and was kept in continued suspense until the cure was completed.

November 3rd.—The child, having been enveloped in a stout sheet, was entrusted to the care of an intelligent dresser, Mr. Hawkes; the projecting incisor was removed, and the lip freely detached from the gum. The angle of one side was grasped with an artery forceps, which it strikes me is the most convenient instrument for this purpose; the edge was removed with a sharp strong scissors; the central piece was similarly treated. These proceedings were then repeated at the opposite side; the parts were then carefully drawn together, and accurately adjusted with three points of twisted suture. In this operation I was aided by the kind advice and assistance of my esteemed colleague, Mr. Wilmot.

4th.—Every thing progressing favourably. Hainsby's truss was applied, to lessen the strain on the needles.

5th.—The wound presents the appearance of recent hemorrhage. The report states that in the course of the night some bleeding came through the right nostril from the upper part of the wound, occasioned by the child sneezing. This was easily controlled, without resorting to any special mode of treatment. The truss was readjusted.

7th.—This being the fourth day after the operation, the needles were removed, the cheeks supported with broad straps of plaster, and Hainsby's truss readjusted. The wound was well united, except along the right border of the central piece.

9th.—The ununited portion of the fissure granulating. This portion and the needle apertures brushed with a ten-grain solution of nitrate of silver.

In a few days the child left the hospital, with the deformity perfectly rectified.

The clinical teachings of this case present to us, I think, three subjects for reflection. First—The advantage of the early operation, as greatly diminishing the amount of trouble attending its performance. Few, unless actual observers, can understand the trouble of managing this restless little patient. Secondly—The necessity, except in very simple cases, of al-

lowing the needles to remain until the fourth day—a practice so ably enunciated by Mr. Butcher, in the pages of this Journal. I am satisfied, had the needles been withdrawn on the earlier date advocated by some high authorities, nothing could have saved this operation from an unsuccessful issue. Thirdly—How trifling a circumstance may, under Providence, interfere with the success of an operation, however skilfully it may have been planned, or however dexterously it may have been executed.

*Strangulated Femoral Hernia in the male.—Operation, and recovery.*

Thomas Healy, aged 62, admitted November 30, 1860. Complaints of obstinate constipation; pain in the abdomen, slightly increased by firm pressure; tympanitis, irritability of the stomach. His pulse is weak and intermittent; in the right groin there is a tumour about the size of a walnut, transversely ovoid, red on the surface, tender to the touch. This tumour is distinctly below Poupart's ligament; not increased by coughing, or diminished by pressure. He states that for twenty years he has had a "small lump" in the groin, for which he could not account, but which caused him no inconvenience. Four days previous to his admission, he was employed shifting some heavy stones; on that evening he passed "some slimy stuff" from his bowels; the next day complained of griping pain in the abdomen. He now observed that the tumour had become enlarged; he had nausea, vomiting, constipation. He continued in this state for two days, when he applied to Mr. Burket, of Tallaght; who, seeing the nature of the case, at once advised his removal to hospital. Being weak after the journey, a little warm wine was administered, and subsequently an enema.

The case was clearly one of strangulated femoral hernia. The tender and inflamed state of the tumour precluded any prolonged or efficient attempts at taxis; accordingly, in consultation with Mr. Colles and Mr. Wilmot, it was determined at once to proceed with the operation. The employment of chloroform was barred by the intermittent character of the pulse. A vertical incision was made by transfixion over the tumour, the layers of fasciæ carefully divided on a director until the sac was reached. This was laid open up to its neck; it contained a little fluid; a portion of omentum much thickened was thus exposed. Careful examination beneath this disclosed a small knuckle of intestine; the point of the finger could distinctly feel

the sharp constriction caused by the crescentic edge of the iliac portion of the fascia lata as it passes to be inserted along with Gimbernat's ligament. Beneath this a fine Cooper's knife was passed; slight *pressure* caused it to yield before the knife. The intestine was now easily reduced, but appeared to be drawn down again by the omentum which was adherent to the outer part of the sac. The adhesion was partly dissected, and a portion of the omentum was cut off; a single vessel required ligature. The intestine, being again reduced, remained so; the wound was dressed in the ordinary way, with a single point of suture, plaster, and a pad. The patient having been removed to bed, a little warm wine and water was given, followed by a full opiate.

December 1st.—Slept during the night. Abdomen soft, free from pain; stomach quiet; pulse weak, but regular; bowels unmoved. To have egg and brandy mixture every second hour. Evening.—Bowels still unmoved; pulse firmer. To have enema of soap and water.

2nd.—Pulse full and strong, tongue dry; some little tenderness about the wound; bowels still unmoved. Omit egg and brandy mixture. Calomel and opium every second hour; half an ounce of castor-oil in peppermint-water as a draught. Evening.—No evacuation from the bowels. The enema to be repeated; warm dressing to the wound. Some scybala passed with the enema, followed in half an hour after by a full motion from the bowels.

3rd.—Bowels moved a second time. Calomel and opium to be repeated twice in the day. To have six ounces of wine.

4th.—Wound still tender; tongue furred, but moist; pulse 90, regular.

5th.—Slept well; no motion from bowels. To have arrow-root.

6th.—No evacuation; tenderness spreading around the wound towards iliac spine. To have three leeches; castor-oil to be repeated. To have stirabout and beef tea.

7th.—Omentum disposed to slough.

17th.—Wound perfectly clean; all tenderness gone; abdomen soft; bowels acting regularly. Omit all medicine; to have chicken.

21st.—Ligature detached; wound healed.

22nd.—Discharged.

Reflecting on the history and progress of this case, we may safely affirm that it was originally a protrusion of the omentum, and that the straining exertion caused a subsequent escape of a knuckle of intestine. I think every candid surgeon will

admit that the operations for hernia which occur in hospital practice are remarkably unsuccessful, even though they enjoy the advantages of the highest operating talent; and this may be explained by the fact, that the majority of the cases which present themselves have been submitted to a long course of treatment in the hope of sparing the patient the pain and risk of the operation. It is only when these means have failed that they are sent to hospital, fairly enough, when we consider the want of proper appliances, and the difficulty of obtaining the requisite amount of care and attention after the operation, which insures its success infinitely more than absurd refinements in the mechanical steps of its performance. If the truth of this statement strikes home to the reason of the surgeon, he may deduce from it the incontestable value of early operation. Far less mischief is likely to accrue from a clean incision, involving even the much-dreaded sac, than the stretching, twisting, and pressure exercised in the process of manipulation, or the lowering of the already too depressed vital powers by the means usually called to the aid of taxis. In this operation the sac was freely divided. This proceeding may draw down the censure of many, the weight of whose authority commands the highest respect; but, reasoning independently for myself from all that I have seen of the operation, and read, as well as from hundreds of dissections of the parts involved, I have found nothing to convince me that a clean division of the peritoneum is as much to be dreaded as all the compression and poking in the dark so often necessary to reduce a hernia through the narrow neck of its sac. A lucid paper has appeared in a late number of this Journal, from the able pen of Mr. Wilmot, whose experience in this operation has been very extensive, in which this matter is forcibly discussed, and placed in a similar light before the profession.

The constriction in this case was caused by the superior external edge of the saphenic opening, sometimes called in this country Hey's ligament, passing to be inserted as described by Mr. Colles, with all the natural truth of that practical writer, along with Gimbernat's ligament, but extending below and external to it. In the division of this structure care was taken to cut by simple pressure, and not by sawing movement. I have frequently demonstrated on the dead subject, in cases of irregular obturator artery, that the fibrous structures may be divided by this method, and the artery borne quite safely on the rounded portion of a Cooper's knife, when, if the sawing movement be employed, it will be divided. The thickened and unhealthy state of the membrane decided us on

cutting it away, in order to diminish the amount of suppuration in the wound.

*Wound by a meat bone, attended with remarkable symptoms.*

James Doyle, aged 29, a butcher, admitted October 25, 1860. Presents all the appearance of extreme prostration. His face is pale and sallow, with an expression of intense anxiety; his pulse is small, and compressible, 110; the surface of the body covered with bullæ, from half an inch to an inch in diameter, some filled with limpid serum, others opaque and puriform, the base surrounded by a red border. His breathing is difficult; the inside of the lips and mouth presents superficial patches, from which the cuticle is detached, but resting loosely on it; his throat is sore, and bloody saliva flows from his mouth. On the middle finger of the right hand is a small suppurating sore, with hard and red margins; the arm and hand are swollen and painful. He states that about a fortnight ago he scratched his finger with a mutton bone; being engaged at the time, he paid no attention to it until an hour after, when, as is usual with the trade, he plunged it into pickle. The day following, he found that the wound was scabbed over; but subsequently some matter formed under the crust, which he punctured with a knife commonly employed for bleeding calves. On the second day after this, he found the wound swollen and red. He applied a poultice, but the swelling extended along the back of the hand. He now felt sick, lost his appetite, but took porter several times in the day. At the solicitation of his friends, he applied to a woman in Church-street, who enjoys considerable reputation for the cure of such sores. She applied some green ointment without any good effect. He was very ill all night, raving and sleepless; and next morning found his hand swollen still more, with great burning pain. He again applied to the herbalist, who advised the ointment to be continued. He now observed red streaks extending up the fore-arm and arm to a hard inflamed spot, above the inner condyle. In this state he continued for two days, gradually becoming worse; the glands in the axilla became inflamed; one or two blisters formed on the painful part of the arm: these he opened, but they soon filled again, and extended over the entire body. He then presented himself for admission. He states that he has been always healthy and sober, never having had venereal; has had two attacks of erysipelas of the head. He was put on tonic treatment; quinia, sulphuric acid, with wine, and sarsaparilla broth.

29th.—The patient presents a truly alarming appearance. The surface of the body has an icteric hue; large bullæ cover

the surface of the body; some have broken, and degenerated into superficial ulcers; the eyelids are excoriated and puffy, with muco-purulent discharge; the nostrils closed with a tough reddish secretion; the breathing difficult. To have sixteen ounces of wine, and half a pint of barm. Oxide of zinc to be sprinkled on the raw surfaces.

30th.—Complains of much soreness in the throat. To have chloride of lime gargle. He continued in this state for some days, when a new train of nervous symptoms set in. He was delirious in the night; he is continually moaning, and rolling his head from side to side on his pillow. If spoken to, he answers in a peculiar loud voice; his respiration is whiffy. Ordered egg and brandy mixture, one grain of calomel on the tongue every second hour.

November 7th.—Distinct evidence of mercurial action on the tongue and gums; delirium less; appears better. Calomel to be administered three times daily.

12th.—Abdomen tympanitic, nausea, urine retained, slight return of the rolling of the head and the delirium. To have turpentine enema; other treatment to be continued.

13th.—Much better; passes water himself. From this report he has continued steadily to improve, although his recovery was retarded by two large bed-sores, which occurred from pressure on the ulcers, notwithstanding every precaution was adopted. His appetite is now amazing. He consumes two chops, half a chicken, two eggs, beef-tea, porter, wine, and a proportionate allowance of bread daily.

If we reflect on the various modes in which this patient was exposed to the contact of animal matter, as well as the symptoms which supervened, we can with difficulty come to any other conclusion than that he received into his blood some poison, which, acting by zymosis on that fluid, produced all this alarming disturbance of the general system. We have, first, the wound caused by bone; secondly, the immersion in brine, in which various kinds of meat not disposed of were, after some time, placed; thirdly, the puncture with the knife, probably stained with animal matter. Making all due allowance for the fallacies of reasoning *post hoc ergo propter hoc*, I think we are justified in assigning blood-poisoning as the cause of the phenomena. The appearances at one time resembled very closely those seen in glanders; but the nasal mucous membrane was engaged only secondarily, and as the result of bullæ forming on its surface, as on the entire tegumentary system. It required a very considerable period to bring the blood to its healthy condition; he was under treatment from October to February following.

ART. XI.—*On the Pathology of Asthma.* By GEORGE H. KIDD, M. D., F. R. C. S. I., Assistant Physician to the Coombe Lying-in Hospital; formerly Lecturer on Anatomy and Physiology in the Dublin School of Medicine.

SIR JOHN FORBES, in introducing the section on the Pathology of Asthma, in his article on this disease in the "Cyclopaedia of Medicine," remarks, with a truthfulness that is, perhaps, as marked now as when he wrote, that "Systematic and theoretical writers, in attempting to explain the phenomena of asthma, seem to have been misled from the truth by partial views of the subject, and by that aim at extreme simplicity that has so often proved a stumbling-block to medical inquirers. In contemplating the animal system in disease, physicians have been too often disposed to overlook the fact, so obvious in health, that it is by a variety of operations, all conspiring to the same end, that results are produced; and, consequently, that if we attempt to explain the operations of the living body, whether in the healthy or disordered state, on any exclusive or isolated principles, we must inevitably fail."

Since Reisseissen demonstrated the fibres surrounding the bronchial tubes, and Dr. Williams proved them to be actually muscular, and capable of contracting under the stimulus of galvanism, writers—English writers, at least—have been unanimous in attributing the phenomena of asthma to the "exclusive and isolated principle" of the spasmodic contraction of these muscles, and have, consequently, it appears to me, failed to render a true account of the disease. So extremely simple has the explanation founded on this principle seemed, that it has, indeed, proved a stumbling-block to inquirers; and we find that systematic writers have followed one another in describing the phenomena as they should occur in accordance with the theory, rather than in applying themselves to ascertain the facts, and allowing the theory to take care of itself. Thus we find that one author after another describes the thorax as being constricted during the paroxysm of asthma, and sounding badly on percussion, and asserts there is a hollow always to be observed at the epigastrium, produced by the atmospheric pressure forcing the stomach and liver up into the thorax, to fill up the vacuum produced there by the vain efforts of the patient to inspire, while the ingress of the air is impeded by the closed, or at least greatly contracted bronchial tubes; and yet I believe this state of the thorax never exists in spasmodic asthma.

Asthmatics are generally "so well up" on their own cases,

that it is very rarely physicians have opportunities of seeing them during the fit. Indeed, Dr. Watson expressly states, in his Lectures, that since he had become acquainted with the value of the stethoscope, he had never had an opportunity of applying it to the chest of a patient labouring under a paroxysm of spasmodic asthma. So we must not utterly blame writers for copying descriptions of the fit from one another, especially as the symptoms described harmonize so well with the simple theory. Circumstances threw me a good deal into contact, many years since, with one who suffered much from asthma,—one who, from having himself been educated for the medical profession, was well able to analyze his sensations; and by him I had my attention early called to the shortcomings of the generally received description of the symptoms. All my subsequent experience has served to confirm my friend's observations; and I now find that the extended researches of Dr. Hyde Salter prove their correctness. I propose, therefore, to draw attention to the symptoms as they actually exist, and to point out the theoretical views as to the pathology of the disease that they directly teach. This has already been done in some measure, in a review of Dr. Salter's work, which appeared in the number of this Journal for August, 1860; but it seems desirable to do it more definitely in a separate communication than could well be done in a review.

The circumstances to which I would then draw particular attention are, that, instead of the thorax being constricted, and the diaphragm and abdominal viscera being drawn up into the chest, causing a hollow at the epigastrium as generally described as occurring during the fit of spasmodic asthma, it is found, on careful observation, that the thorax is distended to the greatest extent,—that it measures from two to three inches more in circumference than during the interval,—that the intercostal spaces are widened,—that the diaphragm is pulled down to the fullest extent, so as to produce great fulness of the abdomen, and cause the heart's impulse to be felt at the scrobiculus. The sounds, moreover, afforded by percussion over the thorax, instead of being dull are found to be clear, and it is evident that the whole chest is full of air; and on stripping the patient, it is seen, that not only are the muscles of ordinary inspiration in a state of violent action, but that those of forced inspiration are also called into requisition, distending the thorax to the utmost. Dr. Salter's description of this is so graphic that I give it at length:—

“On stripping an asthmatic in the height of a paroxysm, an admirable example is seen of the immense array of muscles



that become on an emergency accessory to respiration ; and some idea is formed of the toil of an asthmatic, and the extremity of those sufferings that necessitate for their relief such intense labour. All the muscles passing from the head to the shoulders, clavicles, and ribs, are rigid ; and the head is rendered a fixed point, from which they can act on their respiratory attachments. Ordinarily these muscles, such as the splenii and scaleni, have their inferior attachment fixed, and move the head and neck ; but now their upper attachment is fixed, and from it they act as mediate or immediate elevators of the ribs, or distenders of the thoracic cavity : and this is how it is that the asthmatic is incapable of moving his head. By the contraction of the trapezius and levator anguli scapulæ, the shoulders are raised to the ears, in order that the muscles that proceed from the shoulders to the ribs may act at an advantage as elevators of these latter. The muscles of the back are so engaged in respiration, that they cease to support the trunk, and so the gait becomes stooping. At every inspiration, the sterno-mastoids start out like cords, and produce by their sudden prominence a deep pit between their sternal attachments. . . . . Meantime all the muscles that increase the capacity of the chest are straining their utmost, and starting into prominence at each inspiration. As each breath is drawn, every muscle is thrown out into bold relief ; and since there are hardly any muscles of the trunk that are not mediately or immediately respiratory, the whole muscular system of the trunk may be mapped out in every part of its detail. The straining muscles are rendered all the more conspicuous from asthmatics being generally so thin."

It will be observed from the foregoing—first, that the chest during the fit of asthma is full of air, and distended to the fullest extent ; secondly, that all the muscles of inspiration are in a state of excessive and violent action. We have next to remark that there is great difficulty experienced in expiration. Though Dr. Salter adheres to the "exclusive and isolated principle" that asthma depends altogether on the spasmodic action of the bronchial muscles, and does not recognise the teachings of his own clinical observations, these observations are so accurate and consonant with my own, that I shall quote his description in illustration of this point also :—

"In the most intense asthmatic breathing, the difficulty of getting the air out of the chest is so great, the expiratory movement (with all its effort) so slight, and the quantity of air expelled so small, that, as if aware that the chest would never be emptied at that rate in time for the next respiration,

a violent involuntary effort comes to the assistance of the expiration, and pumps out the remainder of the air with a violent jerk. This termination of a prolonged, ineffectual, and almost motionless expiration, by a sudden expiratory jerk, is characteristic of the intensest asthma, and occurs in no other form of dyspnoea whatever. Whenever we see it, we may be sure that the bronchial spasm is extreme. It is at expiration, too, that the asthmatic experiences the greatest distress."

In the appendix to Dr. Salter's book there are several very interesting cases. One of these has been contributed by the gentleman who early drew my attention to this subject. "The balance seemed quite destroyed," he says, describing his own sensations, "between inspiring and expiring. In spite of myself, I was forced to keep tugging *in* the air with all the muscles and joints of my body, while it seemed as if it hardly went *out* again at all. A *cough* as an expiration was a relief,—but a relief that seldom came in those fits till near the end. Besides the peculiar asthmatic sound of respiration, I used to accompany almost every tug for breath with a sort of groan, which (I imagine) helped me, like the *heck* of the axeman, or served as it best might in place of an expiration proper."

In birds, who have no diaphragm, and who have the ribs connected by osseous prolongations with a sternum so large as to cover almost the entire front of the body, the natural state resembles closely that of the human subject when labouring under asthma. "The natural state of this bony' frame-work," says Carpenter, "is such, that when no pressure is made upon it, the cavity it encloses is in a state of distention; and the state of emptiness can only be produced by a forcible compression of the frame-work, through an exertion of muscular power. In the state of distention, therefore, which is natural to the cavity of the trunk, the lungs are expanded, and fill themselves with air, which they draw in through the trachea; and this condition they retain till, by the action of the external muscles on the bony frame-work, the cavity of the trunk is diminished, and the air is expelled from the lungs and air-sacs, which are again filled as soon as the pressure is taken off." Now, during the fit of asthma, the state of the thorax and the mode of respiration closely resemble that natural to birds. The chest is distended to the fullest extent, being kept so by the spasmodic action of the muscles of inspiration. When the patient desires to breathe, he must first bring a strong voluntary effort to the aid of the muscles of expiration, so as to compress the thorax, and as soon as he relaxes this, the muscles of

inspiration, being in a state of tonic or persistent spasm, expand the cavity again, and cause fresh air to rush in through the trachea. In the healthy state of the human subject, there is always a rest after each expiration. We have first an inspiration, then an expiration following immediately, and then a post-expiratory rest, occupying a time considerably longer than that of the inspiratory and expiratory movements combined. In asthma, this post-expiratory rest is lost; the moment the expiration is completed, the inspiration begins—there is no pause. In fact, as has been already remarked in the review of Dr. Salter's work, it seems plain that the muscles of inspiration are, during the fit of asthma, in a constant state of tonic contraction; and that the great difficulty is to empty the chest, so as to allow of its being refilled with fresh air. For this purpose, the muscles of expiration are constantly contending with those of inspiration, and require the aid of a strong voluntary effort to overcome them. It appears, moreover, that as soon as the volition is suspended, the inspiratory muscles immediately regain the victory,—the chest is again distended; the moment the expiration is completed, the inspiration begins,—there is no pause,—the normal post-expiratory rest is lost. In consequence of the state of distention in which the chest is during the fit, and of the necessity of overcoming the tonic spasm of the inspiratory muscles, it occurs that the relative length of inspiration and expiration is reversed; instead of the inspiration being longer than the expiration, as is normally the case, the expiration is longer than the inspiration,—four or five times as long; and at the same time the respiratory movements are unfrequent, not more than nine or ten in the minute; and from the state of distention in which the lungs are kept, it also arises that the vesicular murmur can only be heard on desiring the patient to make a prolonged and forced expiration, and then inspire quietly, as has been remarked by Lænnec and Williams.

The desire manifested by asthmatics for an abundance of fresh air during their fits, and their violent inspiratory efforts, seem at first sight to be opposed to this theory, but in reality are best explained by it. Owing to the persistent distention of the thorax, the lungs are filled with air, which, from being stagnant, soon ceases to be of avail for purifying the blood, and hence the "*besoin de respirer*" is felt in an aggravated degree.

While an examination of the phenomena of the asthmatic fit proves that the entire muscles of inspiration are in a state of spasmodic action, many circumstances show that the bronchial muscles are also concerned, and are, too, in a state of

spasmodic action. This is most easily demonstrated in the slighter attacks, where, on applying the ear to the chest, it is found that the respiratory sounds are heard in an undulatory sort of way, marking the dilatation and contraction of the bronchial tubes, and that the points of constriction are constantly changing their site.

There are few problems in physiology more difficult of solution than that of the action of the bronchial muscles. When their existence and muscularity were first demonstrated, it seemed natural to regard them as muscles of expiration, serving by their contraction to expel the air from the lungs; but a little consideration shows they cannot take part in this process. Their distribution alone will prove this—they are found in the trachea, and larger bronchial tubes, occupying the spaces where the cartilaginous rings are deficient, in the posterior fourth of their circumference, passing transversely between the extremities of each ring; in the smaller tubes, where there are no cartilaginous rings, they surround the tubes, and may be demonstrated on the finest tubes, but they do not surround the air-cells or terminal portions of the tubes. Now, these air-cells are the essential part of the lungs—they are expansions at the extremities of the bronchial tubes; it is in them that the blood is aerated; they are much larger in capacity than the bronchial tubes, and these tubes are but ducts for conveying the air to and from the air-cells. The air-cells are emptied by the collapse of the walls of the thorax; and it is evident that if the ducts were narrowed or closed by the bronchial muscles while the air-cells were being compressed, the effect would only be to impede or prevent the egress of the air, and perhaps rupture the cells. We must, then, give up the theory that they are muscles of expiration. It has been suggested that they are used merely to carry forward the mucus secreted in the tubes by a sort of peristaltic movement, and moreover to regulate the quantity of air to be admitted into each lobe of the lungs. The necessity for this latter is not very obvious; and it is plain that where there are cartilages in the tubes, the muscles cannot narrow their caliber sufficiently to aid very much in the expulsion of mucus—and any explanation, to be good, must apply to their entire distribution.

The theory as to their use that seems to me to accord with the greatest number of facts is, that they are muscles of inspiration, and, if this can be proved, it will afford a satisfactory explanation of their being associated with the other muscles of inspiration in the production of a fit of asthma. The principal fact on which this theory is founded is the difference between

the duration of inspiration and expiration. On analysing our own sensations, or applying the hand to the chest of a healthy person, it is evident that the movement of expiration is much shorter than that of inspiration; and on applying the ear to the chest, the inspiratory murmur is found to be much longer than that of expiration. Fournet, who attached much importance to the relative length of these two sounds as a means of diagnosis, says that in health the inspiratory sound is five times longer than the expiratory, and for the purposes of clinical observations proposed to name them as 10 and 2. Now, if a volume of air occupies a certain length of time, which we may call ten, in passing into the lungs, and passes out again in the one-fifth of that time, or in a period that we may call two, it is evident it must either be pressed out with greater force than it passes in, or the tubes through which it passes out must be wider than those through which it passes in. One or other of these propositions must be true—

1st—Either the force of expiration must be many times greater than that of inspiration; or,

2nd—The bronchial tubes must be narrower during inspiration than they are during expiration—i. e., the bronchial muscles contracted during inspiration and the tubes narrowed; the muscles relaxed during expiration, and the tubes dilated.

It is well known that the resistance offered to a current of air passing through a tube increases rapidly, in proportion to the velocity of the current. The correct estimation of this resistance is one of the most difficult problems in dynamics. Newton considered that the resistance varied generally as the squares of the velocities of the currents; but it is well known that in small tubes, and with great velocities, the resistance is much greater than this. By a simple experiment, I have attempted to form some estimate of the amount of force that would be required to cause a current of air to pass out of the bronchial tubes five times faster than it passes into them. Having accurately balanced the receiver of Hutchinson's spirometer, I suspended it by a single cord, passing over a pulley; to one end of this cord the receiver was attached, to the other the balancing weights; I then lowered the receiver into the water, and having slightly opened the air-tap I attached weights to the cord till I caused the receiver to rise, drawing air in through the air-tap. I found that a weight of 11lb. caused the receiver to rise so as to draw in 400 cubic inches of air in 60 seconds; and I formed the following table:—A weight of 11lb. caused 400 cubic inches of air to pass through the narrow opening of the air-tap in 60 seconds, and

a weight of 2lb. caused it to pass through in 40 seconds, and so on.

1 lb	.	.	.	60 seconds.
2 „	.	.	.	40 „
4 „	.	.	.	25 „
6 „	.	.	.	19 „
8 „	.	.	.	16 „
9 „	.	.	.	15 „

Showing that it would require the force moving a current of air through such an aperture to be multiplied by 9 to increase its velocity fourfold.

This experiment may be varied so as to cause the velocity of the current to approximate to that of the current passing into the lungs in ordinary breathing. The quantity of air passing into the lungs in ordinary breathing has never yet been determined; but it is estimated by Coathupe, whose experiments are considered most worthy of confidence, at about 20 cubic inches: now, say that this occupies a second, or a second and a half, in passing into the lungs, it is obvious that if we arrange the spirometer so as to draw in 333 cubic inches in 25 seconds, the air will have about the same velocity as that passing into the lungs. I opened the air-tap of the instrument so as to allow the receiver to rise to 333 cubic inches, with a weight of 1lb., in 25 seconds, and then added weights till it rose to the same height in five seconds, when I found I had been obliged to put 10lbs. in the scale.

I infer from these experiments that, to cause the air to pass out of the lungs in the one-fifth of the time it occupies in passing into them, the diameter of the bronchial tubes remaining the same, it would be necessary that the force of expiration should be at least ten times greater than that of inspiration. But it has been proved by the experiments of Mendelssohn and Hutchinson (these last extending over 1500 individuals), that the expiratory force is only one-third greater than the inspiratory force. We thus eliminate the first proposition, and are reduced to the second—that during inspiration the bronchial muscles are contracted, and the tubes narrowed; and during expiration the muscles are relaxed, and the tubes dilated.

The mode in which the contraction of the bronchial muscles assists in inspiration is easily explained. The object of inspiration is to carry a current of air into the air-cells. This is effected by enlarging the cavity of the thorax relatively to its contents—1st, by dilating the cavity; 2nd, by lessening the bulk

of its contents. This latter is effected by the bronchial muscles, which narrow and shorten the bronchial tubes, leaving extra space for the enlargement of the air-cells. The narrowing of the tubes assists inspiration materially in another way. Though in ordinary respiration there be but twenty cubic inches of air introduced into the lungs and expelled again, we know that in health a quantity of air remains in the lungs after ordinary expiration, varying in different subjects between 117 and 430 cubic inches. The greater part of this is lodged in the air-cells, under circumstances most unfavourable for its displacement. By the narrowing of the tubes, the displacing force of the current will be greatly increased, and thus important aid given to the essential part of respiration. So difficult does it appear to effect the displacement of the contents of the air-cells, that it is stated by Dr Walshe, in his treatise on diseases of the lungs, that in calm ordinary breathing the air in the air-cells is stagnant and unchanged, the tidal current never reaching it. On such a supposition, the greater part of the lungs would be useless, or worse, for it would contain impure air—air unchanged, except so far as could be done by the tendency of the tidal air and it to become mutually diffused. The action of the bronchial muscles now described, however, obviates the difficulty; for, by narrowing the tubes, they cause the current to acquire a great increase of displacing power, and this is probably the most important use of these muscles.

Emphysema of the lungs is one of the most constant results of asthma, and can be best accounted for on the supposition that the chest is in a state of distention during the fit. Dr. Gairdner has shown that emphysema is produced by the over-distention of some of the lobules of the lung during inspiration, caused by their swelling out, to occupy the space of other lobules to which the access of air is prevented by a plug of mucus, or other closure of a bronchial tube. We have but to suppose the spasm of the bronchial muscles to be so complete as to close some of the tubes, when the greatly-distended thorax supplies the condition essential for the production of the emphysema.

I have now shown,—

1st,—That during the paroxysm of asthma, the chest is distended to the greatest possible extent.

2nd,—That all the muscles of inspiration are in spasmodic action (tonic spasm).

3rd,—That the bronchial muscles are muscles of inspiration, and associated in the spasmodic action with the other muscles of inspiration.

4th,—That breathing is carried on by bringing a voluntary effort to aid the muscles of expiration; and that as soon as this is relaxed, the muscles of inspiration, like so many stretched bands of Indiarubber, distend the chest again.

I proceed now to trace this spasmodic action to its cause. The "simplicity" of the theory, that the paroxysm depended solely on the spasm of the bronchial muscles, has too often prevented the true cause of the disease being recognised. The following facts may be referred to, as showing that the spasm arises from some morbid action in the medulla oblongata:—

1st,—The fact that the spasm affects an entire group of muscles. Now, Schröder van der Kolk has shown that muscles which are associated in action are supplied by nerves arising from special groups of mutually associated and connected ganglion corpuscles. Disorder of this group would then manifest itself in the entire class of muscles.

2nd,—Van der Kolk has also shown that the skin covering parts moved by muscles, is supplied with sensitive nerves arising from the same segments of the spinal centre, as the motor nerves of those muscles arise from\*. Now, Dr. Salter has remarked, as an almost universal premonitory symptom of asthma, that there is *itching* of the skin under the chin, over the sternum, and between the scapulæ. This, it is evident, is a subjective sensation, and indicates an irritation existing at the roots of these nerves.

3rd,—Paroxysms of asthma are observed to occur in cases of acute hydrocephalus, as in a case mentioned by Dr. Salter, and in one mentioned by Dr. Graves, where there were also general convulsions. In persons liable to epilepsy, recurring at regular intervals, fits of asthma occasionally take the place of, and serve as substitutes for, the epileptic fit.

4th,—The state of the patient prelude the fit of asthma indicates an affection of the nervous centres. In one there is mental exhilaration, in another mental depression. A patient of Sir J. Forbes is awakened from sleep by convulsions in one foot and leg; and as soon as the asthmatic fit is developed, the convulsions of the extremity cease.

5th,—The exciting causes indicate the same. In one, cold water applied to the instep will cause an attack; in another, going to bed with a loaded rectum; in a third, undigested food in the stomach; and in another, sudden emotion; which latter

\* There are many interesting illustrations of this law in a note, at page 7, of the edition of van der Kolk's works published by the Sydenham Society.



will also sometimes check the paroxysm, even when fully developed.

From all these circumstances, I infer that asthma depends on a morbid state of the medulla oblongata and spinal centres, which manifests itself by throwing the entire group of inspiratory muscles into spasmodic action.

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ART. XII.—*Further Remarks upon a new and successful mode of treatment for Vesico-Vaginal Fistula.* By MAURICE H. COLLIS, F. R. C. S., Surgeon to the Meath Hospital.

IN a former number of the "Dublin Quarterly Journal," I drew the attention of hospital surgeons to a new method of operation in cases of vesico-vaginal fistula. I claimed for it some advantages over older methods, which had proved unsatisfactory in their results—advantages which a more extended experience has shown to be its just due, and which the cases I am now about to detail will be found to illustrate. The operation consists, first, in splitting the margin of the fistula all round, so as to separate the vesico-vaginal septum into two equal portions, one-half consisting of the vaginal mucous membrane and submucous tissue, and the other of the vesical mucous membrane and submucous tissue, the muscular portion of the septum being equally divided between the two. The extent of this artificial separation is to be regulated by the extent of the fissure, by the condition of the margins, and to a certain extent by the position of the fissure. Where the fistula is near the vesical end of the urethra, or near the cervix uteri, the dissection need not be carried to any great depth—in the former case from the substantial and highly vascular condition of the parts which insures ready union, and in the latter from the toughness of the structure, and also because there would be some risk of opening into the canal of the cervix, an accident which would give rise to a troublesome complication, in the form of a fistula, most difficult to close. On the other hand, when a fistula is situated in the intervening space or floor of the bladder, the dissection requires to be somewhat more extensive, owing to the thinness of the septum, which would otherwise afford too small an extent of raw surface to secure union.

The condition of the margins regulates the amount of splitting in this way—if the margins are unhealthy, or infiltrated, or unnaturally thinned away, the separation of the

flaps must be proportionately extensive; if they are healthy, a less amount of dissection suffices. The measurement of the fissure has also a bearing on this point, as stated above; if it be extensive, the flaps must be ample; if small, they need not be large.

The second step of the operation consists in the insertion of the sutures; for my operation either wire or thread will answer; silk is now justly condemned by most surgeons as irritating, and productive of ulceration, as a suture. Good thread has always seemed to me preferable to iron-wire. I have not an equal experience of silver-wire, and cannot speak to its merits; but I have frequently found iron-wire as irritating in the vagina as silk, although in cases of cleft palate it will remain for weeks unaltered and harmless. I have even gone to the trouble of securing the same kind of wire as Professor Simpson uses, but without having the satisfaction of obtaining the same immunity from sloughing which it seems to possess in his hands. I cannot account for this fact, and only state it that others may not be disappointed. Thread, on the other hand, seems not to produce much local irritation; and I have had thread-sutures lying in the vagina for ten days without producing injurious suppuration. The needles which I use are known as Liston's needles; they are fixed in long handles, with the eye near the point; with these it is easy to pass the thread; and if wire be used, it can be drawn through the flaps by attaching it to the thread. I have my ligatures about a quarter of an inch from each other, and at a considerable distance from the margin of the fistula; this is of great importance. Each suture consists of a doubled thread; and when all are passed through the flaps, a piece of vulcanized India rubber cord is run through the looped extremities, which are successively pulled tight upon it, and the free ends are then tied over a similar piece of cord at the opposite side of the fissure; the sutures are not tightly drawn. The degree of tightness with which they are to be tied, and the selection of the distance from the margin of the fistula at which to enter them, will test the judgment of the operator, and will most probably insure success or failure as far as he is concerned. If the raw surfaces be tightly drawn together, so that their edges puff up between the India rubber quills, the flaps will slough. The operator must remember that much swelling will inevitably arise in the course of twenty-four hours, and that the effect of tightly confining the flaps between two bars will be to interfere with their nutrition, and bring about their death. In like manner, if the quills are too close to the margins, their

pressure will produce a similar destructive effect. If there be any strain upon the quills, they must produce some slight ulceration where they lie in contact with the vaginal surface; and if the strain be too great, or too close to the margin of the fistula, they will cut through, and cut off the flap. I have seen this happen in other hands, in spite of warning; and it has happened in my own very lately, owing to my using the hemp ligature, which, when wet, contracts most powerfully. I was not aware of this, and was seduced into its use by its beautiful roundness and smoothness, to meet with a result which was by no means agreeable. In my first case I used for quill a piece of soft black bougie, for which, in subsequent instances, at the suggestion of Dr. Thorpe, of Letterkenny, I substituted the India rubber cord. Its advantages are its elasticity, which enables it to give a little in case of unexpected swelling, and its pliability, which enables us to adapt it to any inequalities of the margins of the fistula. This was strikingly illustrated in Madden's case (No. 3), where the rent was irregularly crucial, and where no unyielding bar would have suited.

Such are the steps of my operation; and I shall now briefly explain in what particulars it seems to be an improvement on the ordinary and older method.

*First*—It affords an extent of raw surface far and away greater than where the margin is simply pared. It is manifest that any reasonable extent of surface can be obtained in this way; it is only necessary to extend the ring of dissection, to obtain this end; within reasonable limits there is no objection to this procedure. The ridges which might be supposed to result, and which do in fact exist when the cure is complete, will in a few days be smoothed away, and be productive of no permanent inconvenience.

*Secondly*—This extensive surface is obtained without the loss of a particle of structure, whether in small or large gaps; this is of inestimable advantage. Any one who has removed a ring of mucous membrane from ever so small a fistula, will have observed how large it becomes under the process, and sooner or later will have probable cause to regret the loss of substance thus entailed; whereas, in my operation, if carefully done, no loss of material occurs; and even if it fail, it leaves the patient in no worse condition for subsequent treatment than before. There are, as I shall presently show, cases in which my operation is not expedient, and in these it may be necessary to sacrifice material; but, with these exceptions, I do not think a surgeon is justified in selecting any mode of

operating which necessitates the removal of material where there is already a deficiency, and when other methods, which give at least as good results, are offered for his use.

*Thirdly*—In this operation, we have a double prospect of success. It will be seen, when the raw surfaces are drawn together by quilled suture, that a ridge rises up on the vaginal surface between the quills. A similar and larger ridge is thrown up towards the bladder; this acts as a valve to prevent the water escaping, or even coming in contact with the wound. And even if the parts included between the quills should slough, the flaps which point towards the bladder are exempt from the pressure of the quills and the strain of the sutures, so that they escape from sloughing, and actually unite before the other flaps, and remain united, even if the latter give way.

*Fourthly*—The operation is simple, and requires no very complex armamentarium, and only that amount of dexterity which should be possessed by every surgeon deserving of the name. I generally prefer the lithotomy posture. It admits of the use of chloroform, and in most instances the fistula can be brought into sufficient command. Sometimes, however, I have found it expedient to adopt another position, viz. placing the patient standing with her back to me, and making her lean across a table, as in the position for examination of the rectum. I use Dr. Sawyer's divaricators, which are of strong brass, bent at about four inches from the end, at right angles; two of half inch width for the sides, and one of inch width for depressing the rectum. My knives are slightly bent on the flat, an inch from the point, and only differ from those figured by Simpson in being double-edged. One has a lancet point, the other has the angle rounded off; the former is useful in commencing the incisions, the latter in continuing them. However, if such knives are not at hand, any pocket-case will supply the want; for it is not so much the implement as the hand that guides it that secures success; and all surgeons ought to be able to do their work with as few implements and as simple as possible. A good sharp scalpel will do the work as well, if no more convenient knife should be at hand; and a few common iron spoons, properly bent, will make divaricators at a pinch. I have even known an operator pass sutures with common curved needles held in a dressing forceps, when those with which he had supplied himself became bent or broken in the progress of the operation.

*Lastly*—My operation is suitable to almost every case, and to many cases which could not be subjected to the older methods with the smallest chance of success.

In a large gap, where the loss of substance is to be measured by square inches, no person could expect union by simply paring the edges and drawing them together by the interrupted suture. The strain on the threads would be too great, and they would inevitably cut out. Nor is the success of autoplasmic operations, by which flaps are transplanted from neighbouring parts, such as to lead us to expect much from them. In these cases it is of great importance to have a mode of operating which can be frequently repeated without repeated diminution of the already scanty material. In more than one instance I have succeeded in gradually reducing the aperture to moderate dimensions by a succession of operations, and finally have closed it completely. I have invariably included the entire margin in the first operation, as it is impossible to say what part or how much may unite at once. The advantage of this appeared in one case, by the almost complete closure, at one operation, of a fissure which measured three inches in length; a single point only remained open, and this subsequently contracted so as to cause no further inconvenience. In another case, one-third of a large gap closed at the angles; and a subsequent case of almost equal dimensions was rendered manageable by the centre portion uniting firmly, and so subdividing the fissure into two portions, at which I was enabled to work alternately without loss of time as well as without loss of substance.

In small gaps, on the other hand, it will not redound to the credit of the operator, if the rent is made worse each time that he interferes. Such a misfortune cannot happen by my operation in any case to which it is suited. There is no loss of substance; and the surgeon can begin again *de novo* in a few weeks, with the parts in at least as favourable condition as before.

In comparing it with Bozeman's method of operating, it will be found to possess both advantages and the opposite; but these will be more intelligible when I have explained, as I shall now do, the treatment of a case subsequent to operation. The vagina is washed out with cold water; a catheter is passed for a moment to empty the bladder of any few drops of urine that may have collected, and it is then withdrawn until the patient has been placed in bed. If the fistula inclines more to one side than the other, she is placed on the side that is farthest removed from the fistula, with instructions to the nurse to keep her from turning over on her back, as patients are apt to do while recovering from chloroform. As soon as she is conscious, she is similarly cautioned; and is further advised to lean over as much on her face as she can. In this position she is kept for

at least two days. The catheter is again inserted immediately on her being placed in bed, and a small vessel laid beside her to catch the urine as it drops from her. I have used various catheters, and find the ordinary male gum-elastic the best, for more than one reason—it is long, soft, pliable, and you can insure its cleanliness by having a new one for each case. Further, I have found the urethra less impatient of it than of metal instruments. In any case, it should be carefully watched for the first day, lest a clot of blood should get into the eye and check the flow of water. In case the urine is not seen to drop from it, it should be gently drawn back and forward once; if the urine does not then flow, it should be withdrawn entirely, cleared out, and re-introduced\*. At the end of two full days (forty-eight hours), the patient may be turned carefully on her back, the bed having been wheeled opposite a good light, and with the fingers, or with the gentlest and most cautious use of one or two divaricators, the quills and wound may be brought into view with the smallest amount of disturbance possible. If the wound looks healthy; if the flaps are not blue from strangulation (over-tight sutures); if the quills appear not to have corroded the mucous membrane on which they lie, the fingers and divaricators may be carefully withdrawn, the patient turned on the opposite side, a fresh catheter introduced, and all things left as before.

The next day it will be well to repeat the examination, and, if necessary, to cut the loops of the sutures, or at least of the alternate ones. This is best done by touching them with a sharp knife as they cross the quill, cutting them on the quill in fact, and using no force, no pulling or dragging of the quills, and no too curious inspection to see if union is perfect. On the next day the remaining sutures (if any) are to be divided, and one quill removed if loose; and on the succeeding day the remaining quill and sutures will probably be found loose in the vagina, and may be removed. At each inspection it may be well to syringe out the vagina with tepid or cold water. After the third day the catheter had better be withdrawn, when the urethra will be found patulous. The catheter may be introduced morning and evening, to insure the complete emptying of the bladder; or at any time, if the urine should

\* A little wine should be given as soon as the sickness from the chloroform is passing away; and when the stomach is settled, a grain of opium, in pill, should be administered, and be repeated night and morning; arrow-root or rice should be given as food, and the patient may be allowed to drink as freely as she wishes, the dilution in the urine counterbalancing any increase in its amount. I have found by experience that heroic doses of opium are unnecessary to insure quiet in the bowels, and on one occasion they produced positive mischief by inducing vomiting and diarrhoea.

be felt or seen to cease from flowing. I have found, as a general rule, that it irritates the urethra so much as to induce sup-puration, if the instrument be kept in longer than three days; and the suppuration is apt to extend toward the wound, and may even give rise to subacute cystitis. Hence I recommend and practise its early removal. After the quills have been removed, great care is required for two or three days, lest any accident should rupture the soft union. Vomiting, diarrhœa, incautious movements, and accidental injuries by the catheter or syringe, might do irremediable mischief; but, without some such cause, there is no likelihood of the union giving way. At first, a thickish ridge will mark the site of the line of union, and two lines of raw surface will show where the quills had lain; but in a very few days these will have healed up, and the former will have disappeared. And I think that we may fairly conclude that the ridges on the vesical surface follow the same course. For a variable period incontinence of urine will probably exist. Of this there are two causes—the patulous condition of the urethra, and the loss of dilating power in the bladder. Both are remediable by time and patience; a few words of advice to the patient to make wafer often at first, and by degrees to try and retain a little more in the bladder, will counteract both causes of incontinence. For obvious reasons, the patient should be kept in hospital, or at least should not be allowed to return to her household duties, until the cicatrix has acquired complete solidity; or if such arrangements be not practicable, strong cautions should be given to all parties concerned to avoid for a time everything which could injure it. An example of the mischief of premature coitus will be found in one of the following cases, where, from the circumstances of the case, the usual prohibition had not been given.

As stated above, there are points in which my operation and Bozeman's may be advantageously compared with one another. Where a surgeon possesses judgment, and nicety of touch, he will find my operation afford him excellent results in most cases. He requires both qualities for deciding on the depth to which the splitting process should be carried, the degree of tightness with which the ligatures should be tied, and the moment when to relax them. Bozeman's operation is more mechanical; and, though complicated and troublesome, when once the details are mastered, and the apparatus is bought, it needs dexterity of hand alone to insure reasonable success. For reasons I have given a few pages back, it is as little applicable to large rents as the older method, on which it is an in-

genious improvement. Its great advantage—and to my mind a very moderate one—is the comparative safety with which the sutures can be removed. The shield protects the cicatrix from injury at the moment of their division; and, as they cause no irritation, they need not be removed for several days, until all is perfectly firm. My operation is done much more quickly, and has been completed in periods varying from twenty minutes to an hour and a quarter. I have heard of Bozeman's occupying three hours and a half. There is, however, a form of fistula in which I prefer, and always perform the latter—where the edge of the fistula is funnel-shaped and puckered. Such fistulæ are small, and generally high up. The condition of the margin makes it almost impossible to apply my plan of splitting; and I find it more expeditious and safer to pare away the mucous lining of the fistula and its funnel-shaped mouth, and to insert wire sutures, and apply Hardy's modification of Bozeman's button. In this we have two parallel rows of holes close together; and the ends of each wire, being passed through two neighbouring holes, are simply twisted together, instead of being clamped with shot. I have even applied this plate to the palate in a case of syphilitic loss of substance, in the hope that it would effect a closure. The case was not favourable for success, and only partial improvement followed; but sufficient to show me that most important assistance could be had from a well-adapted button, in suitable cases of fissured palate.

I shall now proceed to detail the cases in which I have operated, chiefly by my own method, and sometimes with Bozeman's. A tabulated arrangement will give the results in a form available at a glance to those who may not care to wade through the history of each case from day to day. It will be seen that some of the patients are still under treatment; but as I wish to give every information, and to leave no loophole for cavilling at the results obtained, I have been perfectly explicit. I have only to add that I have met with but one case in which, though I did not refuse to operate, I thought the chances so small, that I did not press the operation. This was a case of sloughing, which had destroyed every vestige of the floor of the bladder, and left nothing but the sides of the vagina from which any flap could be obtained. The chances of success were next to none, and did not justify me in pressing the operation on an unwilling patient. But, short of this, I believe there is no gap which is so bad that an ultimate cure may not be obtained by patience. Nothing could be more unpromising than some of my cases, and nothing more satisfactory than some of these turned out. One case at present under treatment was apparently quite hopeless when we commenced



operations. The gap was large enough to admit four fingers; the patient a small woman, with small vagina: and yet, after three operations, this huge gap is reduced to a mere slit, of a few lines in length, which I have reasonable hope of closing in another operation. In these large cases, one must be content to cure bit by bit; although, as I said before, it is well to give the entire margin the chance of uniting each time.

CASE I. (Mrs. Giblin, aged 25) is published in the "Dublin Quarterly Journal" for February, 1857. I have to add to the published record that the woman has since been delivered, and that the cicatrix (a transverse one, situated at the junction of the urethra with the bladder) has stood the test of the subsequent parturition uninjured. The cause of the injury was protracted labour in a primipara. The fissure was one inch and a quarter long. The operation was done five or six weeks after delivery, and consisted in splitting the edges of the fissure all round, and uniting the raw surfaces by quilled sutures of common housewife-thread, the quills being pieces of French No. 6 bougie. Ligatures were removed on the fifth day, when union was complete. She speedily gained perfect control over the bladder.

CASE II.—Mrs. Balfe, aged 40, also published in the same number of the "Quarterly Journal," was one of the most satisfactory cases I have had, so far as the amount of relief obtained from the utmost misery. This woman had a rent three inches long, extending obliquely from a point close to the meatus, back to the immediate neighbourhood of the os uteri. Through this chasm the upper wall of the bladder prolapsed to such a degree as to appear externally. She could neither sit, stand, nor walk without pain: as her husband expressed himself to me, she lay for two years on a sack at the fire, not able to do a hand's turn for herself or any one else. In one operation this large rent was closed, with the sole exception of a small hole at its posterior angle. She experienced such instant relief from her acute sufferings, that she refused to undergo a second operation for the closure of this small pinhole, and her subsequent history has proved that she was not far wrong. I have heard from her within the last month, and she states that she suffers no inconvenience, and that, unless after a long journey, she has perfect control over her bladder. The operation in this case was the same as in No. 1. The causes were prolonged labour and delivery with the forceps. It was her sixth child.

CASE III.—Bridget Madden, aged 30, a soldier's wife. She stated that a surgeon (Scotch) made an incision with a razor

into what she thought were the membranes, but in reality into the vesico-vaginal wall. Whether this be true, or one of many inventions of a not very truthful female, I cannot say. Certainly there was a rent of irregular outline, and, taken with cicatrices which existed in its neighbourhood, it bore every appearance of a crucial incision having been deliberately made in this situation. I operated on her four times in February, March, May, and June, 1857. The first, second, and fourth by my own method, and the third by Bozeman's. Finally, she was dismissed perfectly cured, and able to retain her water for three hours at a stretch. Sir P. Crampton and Dr. Beatty, among others, saw this woman, and were much pleased with the result. However, in a few weeks, she presented herself to Dr. Johns during my absence from town, and complained that she was as bad as ever. Upon examination, this proved to be true, in so far that there was an opening in the centre of the cicatrix, of an oval form, about as large as a No. 10 catheter, and with edges no thicker than paper. Careful inquiry proved that she had taken up with another soldier, her proper husband being in India, and in this way the rent had occurred. On my return she applied to me, but under the circumstances a fresh operation appeared not likely to have better success, and I declined any further interference at that time. I have heard nothing of her since.

CASE IV.—Mary Levey, from Portarlington, sent up to me by my friend Dr. Tabuteau. This woman had a rent made by the violence of a midwife in her first confinement. She had six children before I saw her. It was a small transverse rent in the floor of the bladder; the walls were thin. I reduced it by my first operation to the size of a crow-quill; by a second operation it was further diminished, and became so small that I thought to close it by a simple wire suture. This failed, as did also a hare-lip suture over a short small needle; and finally, I tried Bozeman's button with equal want of success. I attribute these failures to overmuch haste in repeating the operations before the edges of the fistula were restored to a healthy state. At last our patience became mutually exhausted, and she left me with a pinhole so small as only to admit of one of Anel's probes. I had tried cautery and caustics to close this, but without avail. Yet this minute fistula was sufficient, from its situation in the centre of the floor of the bladder, to render her as unable to retain her water as the larger aperture with which she first applied to me. She has since had a miscarriage and a labour at term, and expresses to Dr. Tabuteau her determination to come up again when the

child is weaned, so that I may have one chance more of rescuing this from the list of failures.

CASE V.—Catherine Murphy, from Dingle, sent up to me in 1859, by Dr. Alton, of Tralee, to whose kindness I am indebted for the previous details and subsequent history of the case. This poor woman was delivered in a remote part of the country, unaided, and suffering from puerperal convulsions. She recovered with sloughing of the vagina, and occlusion of the passage high up, together with a very considerable vesico-vaginal fistula. I operated on her after my own method on the 11th May, 1859, and as usual removed the sutures on the fourth and fifth days. Union was then perfect; but by an error of the nurse she got an over-dose of opium which brought on violent vomiting. The bowels also gave way at the same time, and severe diarrhœa occurred on the evening of the fifth day; hence a great portion of the soft cicatrix separated, and we had to begin afresh. Early in July I again operated, and this time by iron-wire sutures and Hardy's modification of Bozeman's button. The result of this second operation was so far favourable, that Mrs. Murphy could retain her water for hours while quiet, or for some minutes while moving about. There was still, however, an opening the size of a grain of duck-shot. She left for the country; and circumstances brought me down to Tralee in the ensuing September, when I saw her at Dr. Alton's house. We then cauterized the fistula, which was small and puckered, and hard to discover among the folds of the vagina. I did not see her again, but Dr. Alton writes me that she called on him some time afterwards, and the orifice appeared to be completely closed and a cure effected, as she told him she could retain her water very well, and there was no stillicidium. Her clothes and person had no urinous odour. Now for her subsequent history (I quote from his letter):—"Some months after she called upon me again, and at this visit only to determine the fact of pregnancy, or not. You may recollect at the period of our seeing her here that the vagina was completely closed, with the exception of a very minute orifice, sufficient barely to allow of the introduction of a bougie, and there was no possibility of introducing the finger into the upper portion of the vagina, so as to reach the os uteri. However, upon the occasion of this visit, I found her three months pregnant, the vesico-vaginal fistula seemed completely closed, the site was marked by a very small pucker, as if all the coats were not quite united. She stated she was never wet, but passed the urine a little oftener than she used before her confinement, or when she was quite well. On passing the

finger I could feel that the vaginal surfaces were more apart and bands becoming elongated, but still could not reach the os uteri. I directed her to call again, which she did in November last. She seemed to be cured of the fistula, as no aperture was now left. I then found that at the right side these bands were so elongated that I could introduce my finger into the upper chamber, and distinctly feel the os uteri and surrounding parts uninjured. The parts were very moist, but no urinous smell either from the dress or my finger when withdrawn. She asked me to take her under my care in the approaching confinement, &c. However, circumstances prevented this, and she was delivered under the care of Dr. Williams, of Dingle, without difficulty, after a two hours' labour. The child was dead, and commencing to decay. All went well for two days, when diarrhœa and vomiting set in, and she finally sank on the fourteenth day."

CASE VI.—An elderly woman at Parsonstown, under my friend Dr. Woods. This was a puckered cicatrix, with tough edges of long standing. I operated upon this woman by Bozeman's method, and regret to say that it appears to have totally failed. I never saw the patient except the day of the operation, but the subsequent treatment—viz., perfect repose of bowels (by opium) and of bladder by use of catheter, was fairly and fully carried out by Dr. Woods, and also the removal of the button at the end of ten days. Long before that, however, it was evident that union had failed to occur, as water made its escape *per vaginam*.

CASE VII.—Mrs. Ryan, Limerick, sent to me by Dr. Vernon Russell, with whom I saw her in consultation before she came to town. This was a small fistula near the external parts, nearly a urethro-vaginal fistula. It was readily brought into view. The woman was in excellent health, and I thought it as fair a case as could possibly be to obtain union by the simple interrupted wire sutures. With one sweep of the knife I cut out the margin of the fistula, removing it as a perfect ring. I then inserted three sutures of iron-wire at about three lines distance from each other. The little wound lay in perfect contact and we all congratulated ourselves on the certainty of success. Great was my dismay to find on the third day that water was escaping; and, on inspection, the centre wire was found to have cut its way out like a knife, although the suture had been twisted by no means too tightly. The others followed suit, and we had the satisfaction of ultimately dealing with a fistula double the original size. As soon as parts were well healed and pliable, I operated by my own method—five sutures were required. On inspection, forty-eight hours after

operation, I found a tendency to ulcerate underneath the quills, and I divided the ligatures at one side, relaxing all, but removing nothing. Next day I found the anterior quill loose, and removed it; and the following day I removed the remaining quill and sutures. All had healed, and in a few days she was safe and able to move about. She went home perfectly well, and has so continued ever since.

CASE VIII.—Mrs. Cush, of Dungannon. A formidable chasm, exposing the upper wall of the bladder and the urethra posteriorly. I had immense difficulty in bringing the sides together at all by seven sutures (quilled). I was satisfied to get three of these to hold, and so to reduce the gap one-half, or nearly so, in diameter. This woman has gone back to the country for six months, and I merely give her case lest I should be accused of omitting failures. She might, however, without straining my professional conscience, be claimed as still under treatment.

CASE IX.—Mrs. Hill, Dublin, aged about 30. Small woman. Small vagina. Prolonged labour with instruments, 1854. Came to me in the latter end of 1860, with almost complete loss of the floor of the bladder. The gap admitted four fingers. It seemed a very hopeless case. I operated on her, however, and with great difficulty succeeded in bringing the two sides in fair contact by an obliquely transverse line of union. The right end of the quills was near the external parts, and the left went back more into the vagina and near the os uteri. Dr. Beatty, and several other accoucheurs, saw this operation; and the universal opinion of the bystanders was, that there was little chance of improvement. On the fourth day I had to let go several of the threads, as ulceration was imminent under the quills, from the great strain upon them. Unfortunately, I removed the remainder on the fifth day; and although the greater part of the fissure had united, yet the union was too soft to hold, and in the end, a bar of an inch and a quarter in the centre of the line of union held on, and became firmly consolidated. As a remarkable effect of the operation, I may mention that menstruation, which had ceased to manifest itself for six years, became re-established. One of my sutures had traversed the anterior lip of the os uteri, and the obliterated canal became in this way restored. The woman had suffered severely at each menstrual period, but from this out her sufferings ceased. I sent her away for some months; and on her return in February, 1861, I found the centre bar about an inch in length, shortened up, and firm. The remainder of the huge rent was thus cut up into two portions: the right was funnel-shaped, and would just admit the tip of the little finger; the

left rent was irregular, and about double this in the long diameter. I operated on the latter by my own method, and reduced it to a slit, which I purpose attacking in a few days. The former I operated on by the button method, and have closed it. Thus, in three operations this formidable chasm has been reduced to a mere slit; and the patient is herself conscious of the great improvement that has been obtained by an increase to her comfort, and by a certain though very limited amount of control over her bladder. Formerly she was wet both sitting and lying, as well as standing; now she can retain her water both when sitting and lying for some hours, and even standing, for a few minutes. Such a case as this would encourage me to operate on almost any case however apparently unpromising.

**CASE 10.**—The last case I have to enumerate is that of Mrs. Jordan, at present under treatment for a small fistula, which I have reduced to about the size of a crow-quill, and am trying to close by cauterization. I have operated on her both by my own method and by Bozeman's. My own was spoiled by the new hempligature, which I unfortunately used, and it cut off my flaps. I then tried Bozeman's plan, and have reduced it, as above stated.

To sum up the results of my ten cases:—I had three cured by the operation before leaving me—Cases I., III., VII., all by my own method.

**CASE V.** all but closed on leaving hospital, was finally cured by one touch of caustic. In this the cure by my method would have been complete but for diarrhœa.

**CASE II.** was relieved of all distressing symptoms by one operation after my method; and although a pin-hole seems to remain, she has no stillicidium, unless after a long journey.

**CASE IV.**—Fistula reduced to a minimum by my method, but patient unrelieved.

**CASE VI.**—Failure. Bozeman's operation.

**CASE VIII.**—Partial closure of large chasm by my operation.

**CASE IX.**—Under treatment. Reduced from two inches, square, to a slit of a few lines in length.

**CASE X.**—Under treatment. Reduced one-half.

Five have been restored to comfort; one has absolutely failed by Bozeman's method; two have had their fistulas considerably reduced in size; and if they return, may derive still further benefit, and like the remaining two, still under treatment, may have a fair chance of final cure.

I shall now throw the cases into a tabular form, that the results may be seen at a glance.

Name and Residence.	Age.	Date of Fistula.	Size of Fistula.	Probable Cause of Fistula.	Situation of Fistula.	Complications.	Primipara, or otherwise.	Date of Operation.	Nature of Operation.	Date of Removal of Apparatus.	Result as regards Fistula.	Result as regards Bladder, &c.
Giblin, Naven.	25	February, 1856.	1½ inch. in length.	Seventy hours in labour.	Neck of bladder.	.....	Prim.	Mar. 31, 1856.	Collis'.	Five days.	Cure.	Cure subsiding after subsequent deliveries.
Baile, County Meath.	40	March, 1855.	3 inches in length.	Four days' labour, forceps.	From meatus to os tinea.	.....	Sixth child.	Sep. 24, 1854.	Collis'.	Do.	Pinhole.	Complete, except after a journey.
Madden, Co. Armagh.	30	1855.	Irregular, 2½ by 4 in.	Crucial wound, by razor.	Floor of bladder.	.....	Prim.	Feb. 4, 1857. Mar. 5, 1857. May, 1857. June, 1857.	Collis'. Collis'. Bozeman's. Collis'a.	..... ..... ..... .....	Reduced. Reduced. No improvement. Cure.	Complete until the rupture of the cicatrix, in three weeks, from which
Levey, Portarlinton.	30	1847.	1 inch.	Violence of midwife.	Floor of bladder.	.....	Prim., six children since.	Oct. 26, 1857. Nov. 17, 1857. Dec. 4, 1857. Dec. 17, 1857.	Collis'. Collis'. Hare-lip needle. Bozeman's.	..... ..... ..... .....	Reduced to size of crowquill. Size of probe. Enlarged. Pinhole.	No improvement; subsequent parteritions have not increased the fistula.
Murphy, Tralee.	..	1859. I believe.	1 inch.	Prolonged labour.	Floor of bladder.	Adhesions of vagina.	Not known.	May 11, 1860. June 1, 1859. Sep. 1859.	Collis'. Bozeman's. Caustic.	4th and 5th days. 10th day. .....	Cure, but torn by vomiting. Pinhole. Cure.	Complete.
Ryan, Limerick.	30	.....	4 lines in diam.	Pressure of head of fetus.	Neck of bladder.	.....	.....	.....	Stimpeon's. Collis'.	In 8 days cut out. 48 hours.	Failure. Cure.	Complete.
Parsonstown.	50	Many years.	½ inch. in diam.	Protracted labour.	Floor of bladder.	.....	.....	Sep. 1859.	Bozeman's.	10 days.	Failure.	.....
Hill, Dublin.	22	1864.	2 inches square.	.....	Floor of bladder.	Impervious canal of cervix.	Prim.	Nov. 1860. Feb. 1861. Mar. 1861.	Collis'. Collis'. Bozeman's.	4th and 5th days. Do. 10 days.	Centre bar of 1½ inch united. Portion operated on reduced to a slit. Portion operated on cured.	Great improvement. Under treatment.
Cush, Dungannon.	..	.....	Immense.	Prolonged labour.	Floor of bladder.	Adhesions of vagina.	.....	.....	Collis'.	6th day.	Reduced one-third.	Gone to the cure.
Jordan, Leixlip.	24	.....	½ inch. in diam.	Prolonged labour.	Floor of bladder.	.....	Prim.	.....	Collis'. Bozeman's.	4th day. 12th day.	Failure. Reduced.	Under treatment.

ART. XIII.—*On the Phenomena of Diabetes Mellitus*. By the Rev. SAMUEL HAUGHTON, F. R. S., Fellow of Trinity College, Dublin.

THE history of this remarkable disease affords an instructive example of the slow growth of human knowledge with regard to questions of physiology. The name diabetes (διάβητης, a syphon) was first mentioned by Aretæus, the Cappadocian, as a title of the class of diseases in which diuresis was a prominent symptom, either constant or occasional. He appears to have included dropsy, of various kinds, under the title of diabetes; and there can be no doubt but that the disease now called diabetes mellitus would have been placed by him under the same head. His reason for the disease having been named diabetes is thus stated:—"τῇδὲ μοι δοκεῖ καλεῖσθαι διάβητης ἐπὶ κλησιν ὁκοῖόν τι διαβήτης ἔων, οὐνεκεν ἐν τῷ σκῆνεί τὸ ὕγρὸν οὐ μίμνει, ἀλλὰ ὅπως διαβάθρη τῷ ἀνθρώπῳ ἐς ἐξοδὸν χρεῖται."—*Lib. i. chap. 2*.

Aretæus had no suspicion that sugar, or any abnormal element, was contained in the urine of diabetic sufferers; and this leading and prominent fact appears to have remained unnoticed until it attracted the attention of the learned and observant Dr. Thomas Willis, of Christ Church, Oxford, who thus states the fact:—"Quod autem plerique authores potum aut parum aut nihil immutatum reddi afferunt, à vero longissime distat: quoniam urina in omnibus (quos unquam me novisse contigit, et credo ita in universis habere) tum a potu ingesto, tum à quovis humore in corpore nostro gigni solito, plurimum differens, quasi melle aut saccharo imbuta, mire dulcescebat."

It has been subsequently ascertained that the sugar of diabetic urine is invariably grape sugar ( $C_{12}H_{12}O_{12} + 2HO$ ), which is not a natural or normal product of the urine in health; and that some of the usual natural products of the urine are present in exaggerated proportion. This is now known to be true in particular of the urea, which was formerly thought to be complementary to the sugar, to increase as the latter diminished, and *vice versa*. In 1832, Sir Robert Kane, then Professor of Chemistry to the Apothecaries' Hall, published the results of five cases in which he had determined the amount of urea in diabetic urine<sup>b</sup>, and expressed the opinion that the urea and sugar in diabetes were not complementary; and that in a given time there was probably as much urea excreted as in health. The following are the results he obtained:—

<sup>a</sup> *Vide Lib. De Medicamentorum Operationibus*, p. 64, published at Amsterdam, in 1682.

<sup>b</sup> *Dublin Journal of Medical and Chemical Science*, vol. i. p. 16.



TABLE I.—*Kane's determination of Sugar and Urea in Diabetes Mellitus.*

Case.	Sugar in 1000 parts.	Urea in 1000 parts.	Specific gravity.
1	47.0	9.0	1032.00
2	31.5	9.5	1030.25
3	60.0	6.5	1036.25
4	51.0	5.3	1033.00
5	70.0	13.5	1050.50

The quantity of urine per day passed by case 5, is stated as 10 pints, which would give **170** grains of urea per day. This quantity, although not equal to the average of health, is more than was supposed at the time to be excreted. It is now well known that the quantity excreted is much greater than that excreted in health. In Dr. Parkes' recent work on Urine,\* the urea passed per day in twelve cases, given by various authorities, ranges from 421 grains to 1,411 grains, and has a mean of **811** grains per day. My own observations quite confirm these results.

Much difference of opinion exists as to the quantity of phosphoric acid passed by diabetic patients, the general impression being that it is but little altered. In this opinion I cannot concur, as I have invariably found it greatly increased; this is shown by the following table:—

TABLE II.—*Amount of Phosphoric Acid passed in Diabetes Mellitus.*

Case.	Phosphoric acid per day.		Total.	Urea per day.
	With earths.	With alkalis.		
Keogh, . . .	76 grs.	132 grs.	208 grs.	750 grs.
M'Nee, . . .	24 "	49 "	73 "	1202 "
Murphy, . .	38 "	56 "	94 "	1559 "

If diabetes be a disease accompanied by rapid and great change of tissue, nothing can be more natural than the in-

\* Page 341.

creased excretion of urea, phosphoric acid, chlorine, and all the other healthy constants of urine. I shall reserve, however, all speculation on the physiology of diabetes for the concluding portion of this paper, as I am anxious to keep quite distinct the facts I have observed, and the inferences I think may be drawn from them.

It is commonly supposed that the specific gravity of the urine affords a means of determining the quantity of sugar, without the trouble of chemical experiments. The extent to which this is true may be inferred from the following Table, in which I have collected together the specific gravities of the various diabetic urines, determined by myself with the specific gravity bottle, and also the quantity of sugar per fluid ounce, found by direct experiment from the same specimens of urine:—

TABLE III.—*Comparison of Specific Gravity of Diabetic Urine with the quantity of sugar per ounce*

Specific gravity.	Sugar per ounce.	Name of patient.
1007·7	1·2 gra.	M'Cabe.
1031·8	33·6 "	Murphy.
1033·1	27·3 "	M'Nee.
1033·8	33·6 "	Murphy.
1033·8	31·2 "	"
1034·0	31·2 "	M'Nee.
1035·0	40·9 "	Murphy.
1035·4	36·4 "	M'Nee.
1035·7	33·6 "	Murphy.
1035·9	36·4 "	M'Nee.
1038·1	32·0 "	Keogh.
1038·3	39·8 "	Murphy.
1040·1	39·8 "	"
1040·2	39·7 "	"
1040·6	39·7 "	"

From this Table it appears that if only an approximation be required, for specific gravities between 1030 and 1040, there is no great error in assuming the excess above 1000, as the number of grains of sugar in each ounce of diabetic urine.

In the following cases of diabetes, I endeavoured to ascertain carefully the composition of the food consumed, with regard to its power of producing urea and glucose, so as to be

able to compare the food ingested with the excretions. In order to ascertain the composition of the food, I made the following experiments, in addition to those detailed in my paper on the "Natural Constants of the Healthy Urine of Man".

*Additional Experiments on Food.*

1. *Brown Bread*, used in the Meath Hospital.

200 grs., dried at 212° F., gave 155.00 grs.

26.4 grs. of these gave 4.26 grs. of platinum.

Solids = 77.50 per cent.

Nitrogen = 2.30 "

From these data, I find the following chain, to determine the quantity of urea due to 1 lb. of this bread:—

	1 lb. brown bread.
1	7000 grs. "
1000	775 grs. at 212° F.
1000	23 grs. nitrogen.
28	60 grs. urea.
<hr/>	
	267 grs. urea.

This is a much larger proportion of urea than I found producible from white bread, even of the best quality,—1 lb. of which only produces 196 grs. of urea.

2. *Rice*, used in the Meath Hospital:—

100 grs., dried at 212° F., gave 85.3 grs. solids.

23.0 grs. of these gave 3.10 grs. of platinum.

From which I deduce:—

Solids = 85.30 per cent.

Nitrogen = 1.92 "

The chain to determine the urea producible from 1 lb. of this rice is the following:—

	1 lb. rice.
1	7000 grs.
1000	853 grs. at 212° F.
10,000	192 grs. nitrogen.
28	60 grs. urea.
<hr/>	
	245 grs. urea.

3. *Porter*. For the following analysis of two kinds of Dublin porter, I am indebted to Dr. Apjohn:—

A. 1000 grs., dried at 212° F., gave 57.9 grs. of solids.

35.6 grs. of these, burned with soda lime, gave 3.40 grs. of sal ammoniac.

B. 1000 grs., dried at 212° F., gave 84.8 grs. of solids.

35.4 grs. of these gave 2.30 grs. of sal ammoniac.

• Dublin Quarterly Journal of Medical Science, vol. xxx., pp. 2, 8.

From these experiments, I find :—

A.

	1 quart porter.
4	70000 gra. „
10,000	579 gra. at 212° F.
356	84 gra. sal ammon.
54	14 gra. nitrogen.
28	60 gra. urea.

53.76 gra. urea.

B.

	1 quart porter.
4	70000 gra.
10,000	848 gra. at 212 F.
354	23 gra. sal ammon.
54	14 gra. nitrogen.
28	60 gra. urea.

53.57 gra. urea.

The agreement between these two results is remarkable, and proves that in good Dublin porter the quantity of nitrogen is independent of the solids present, which may vary considerably according to the proportion of the other constituents present.

4. *Tea*, used in the Meath Hospital. The tea given to the patients in this hospital is made in the proportion of one drachm to the pint, or one ounce to the gallon. On analyzing this tea leaf in the usual manner, I found—

70.6 gra., dried at 212° F., gave 60 gra. of solids.

26.3 gra. of these gave 8.70 gra. of platinum.

Solids = 85.00 per cent.

Nitrogen = 4.73 „

From these data, I deduce the following chain :—

	1 lb. tea.
1	7000 gra.
100	86 gra. at 212° F.
10,000	473 gra. nitrogen.
28	60 gra. urea.

603 gra. urea.

5. *Boiled Cabbage*, used in Meath Hospital.

965 gra., dried at 212° F., gave 86.6 gra. of solids.

28.2 gra. of these gave 8.35 gra. of platinum.

Solids = 8.97 per cent.

Nitrogen = 4.23 „

From these data, the following chain is deducible :—

	1 lb. cabbage (boiled).
1	7000 gra.
10,000	897 gra. at 212° F.
10,000	423 gra. nitrogen.
28	60 gra. urea.

57 gra. urea.

The quantity of glucose sugar producible from the various kinds of food used, may be thus estimated:—

1. *Brown Wheaten Bread.* The quantity of urea due to a pound of this bread has been already determined by direct experiment, p. 320; and the sugar producible from it may be found from the following considerations:—

I. According to Peligot's researches, it appears that in whole wheaten meal, on the average, the proportion of proteinic to starch compounds is as 14 per cent. to 68 per cent.

II. The proteinic compounds are to urea as 3 to 1.

III. Starch will produce glucose, in the proportion of 81 to 99.

From these data the subjoined chain follows:—

$$\begin{array}{rcl}
 & \swarrow & 1 \text{ lb. brown bread.} \\
 1 & \swarrow & 267 \text{ gra. urea.} \\
 1 & \swarrow & 8 \text{ gra. proteinic compounds.} \\
 14 & \swarrow & 68 \text{ gra. starch.} \\
 81 & \swarrow & 99 \text{ gra. glucose.} \\
 \hline
 & & 4755 \text{ gra. glucose.}
 \end{array}$$

Hence 2 lbs. of this bread are equivalent to 9510 grains of glucose.

2. *Rice.* According to Boussingault, rice contains 88 portions of starch to 7.5 of azotised matter; combining this with the result obtained by myself, I find:—

$$\begin{array}{rcl}
 & \swarrow & 1 \text{ lb. rice.} \\
 1 & \swarrow & 245 \text{ gra. urea.} \\
 1 & \swarrow & 8 \text{ gra. azotised matter.} \\
 75 & \swarrow & 880 \text{ gra. starch.} \\
 81 & \swarrow & 99 \text{ gra. glucose.} \\
 \hline
 & & 9942 \text{ gra. glucose.}
 \end{array}$$

Hence 2 ounces of rice are equivalent to 1243 grains of glucose.

3. *Milk.* I have found that the Dublin milk contains lactose, equivalent to glucose, in the proportion of 177 grains to the pint. Therefore two quarts of milk are equal to 708 grains of glucose.
4. *Porter.* It has been found that 1000 grains of Dublin porter contains three grains of glucose sugar.\* Hence the sugar in one quart of porter is thus found:—

$$\begin{array}{rcl}
 & \swarrow & 1 \text{ quart porter.} \\
 4 & \swarrow & 70,000 \text{ gra. „} \\
 1000 & \swarrow & 3 \text{ gra. glucose.} \\
 \hline
 & & 52.5 \text{ gra. glucose.}
 \end{array}$$

\* Dublin Quarterly Journal of Science, Vol. I. No. 2.

Combining together the results of all my experiments, I have constructed the following table:—

TABLE IV.—*The Urea and Sugar Equivalents of several kinds of human Food.*

No.	Food.	Quantity.	Urea Equiva- -lent.	Sugar Equi- -valent.
1	<i>Lean roasted mutton</i> ; leg, near shank end; contains fat and fascia . . . . .	1 lb.	480 grs.	
2	<i>Lean roasted mutton</i> ; leg, near the loin; very little fat or fascia . .	1 lb.	957 "	
3	<i>Lean raw mutton</i> , loin . . . . .	1 lb.	544 "	
4	<i>Lean roasted beef</i> , sirloin . . . . .	1 lb.	724 "	
5	<i>Lean raw beef</i> , sirloin . . . . .	1 lb.	979 "	
6	White bread, first quality . . . . .	1 lb.	196 "	
7	White bread, second quality . .	1 lb.	203 "	
8	Brown bread, wheaten . . . . .	1 lb.	267 "	4,755 grs.
9	Oatmeal . . . . .	1 lb.	421 "	
10	Indian meal . . . . .	1 lb.	150 "	
11	Rice . . . . .	1 lb.	245 "	9,942 grs.
12	Cauliflower, boiled . . . . .	1 lb.	58 "	
13	Cabbage, boiled . . . . .	1 lb.	57 "	
14	Tea . . . . .	1 lb.	603 "	
15	Porter . . . . .	1 qt.	54 "	
16	Milk, sp.gr.=1025 . . . . .	1 qt.	116 "	354 grs.
17	Milk, sp.gr.=1027 . . . . .	1 qt.	126 "	
18	Milk, sp.gr.=1035 . . . . .	1 qt.	174 "	

From this table it appears that one pound weight of lean beef or mutton is capable of producing from 500 to 1000 grains of urea, according as its muscular fibre is more or less mixed with cellular fat. The highly nutritious qualities of oatmeal,

wheaten bread, and rice, as compared with Indian meal, are also apparent; and the agreement between the cabbage and cauliflower is natural.

The first case of diabetes mellitus that I shall describe is that of Owen Murphy; for the opportunity of observing this case I am indebted to the kindness of Dr. Stokes, under whose care he was in the Meath Hospital. The history of his case was taken by Mr. A. W. Foot, one of Dr. Stokes' pupils, whose zeal and intelligence are highly to be commended. I have added to my own remarks the statement of Mr. Foot, which renders any reference to details useless on my part.

Owen Murphy came under my observation in the beginning of November, 1860, and died on the 14th January, 1861. Tubercle was first detected in his right lung on the 27th November, 1860.

The following Table contains an account of his weight (naked), urine, its specific gravity, and the quantity of urea and sugar it contained; the specific gravity of the urine, its sugar, and urea, were observed on a mixture of all the urine passed in the 24 hours:—

TABLE V.—*Constants of Body and Urine of Owen Murphy, 1860–61.*

Date.	Weight.	Urine.	Sugar per day.	Urea per day.	Specific gravity.
1860, 8th Nov.	98 lbs.	310 3 fl.	12,329 grs.	1559 grs.	1040·2
„ 15th „	92·5 „	363 <sup>a</sup> „	12,216 „	1429 „	1033·8
„ 22nd „	93·5 „	360 „	11,250 „	1420 „	1033·8
„ 29th „	95·5 „	300 „	12,290 „	1181 „	1035·0
„ 6th Dec.	96·5 „	275 „	9255 „	1203 „	1035·7
„ 13th „	—	240 „	9545 „	1181 „	1040·6
„ 20th „	93·5 „	175 „	6960 „	976 „	1038·3
1861, 3rd Jan.	91·5 „	194 „	7716 „	849 „	1040·1
„ 10th „	87·5 „	190 „	6394 „	840 „	1031·8 <sup>b</sup>
Mean, . . .	<b>93·56</b>	<b>267·4</b>			

<sup>a</sup> Gives off a vinous smell, and carbonic acid gas.

<sup>b</sup> This sp. gr. is not to be depended on, as carbonic acid gas was being rapidly evolved from the fermenting urine during the weighing.

The quantities of urea and sugar ingested are found from the following calculations, based on the quantities of food and its composition already determined:—

**UREA EQUIVALENT OF FOOD.**

**1.—Food per day previous to 8th November, 1860.**

1. Brown bread, 2 lbs.,	equivalent to 534	grs. urea.
2. Beef, $\frac{3}{4}$ ths of lb.	207	„
3. Rice, 2 oz.,	31	„
4. Milk, 2 quarts,	232	„
5. Porter, 1 quart,	54	„
6. Tea, 1 pint,	5	„
7. Eggs, 2,	90	„

---

**1153** grs.

**2.—Food per day previous to 15th November, 1860.**

Same as No. 1. Food changed to following on 16th November.

**3.—Food per day previous to 22nd November, 1860.**

1. Brown bread (toasted), 2 lbs.,	534	grs. urea.
2. Beef, 1 lb. (boiled),	724	„
3. Rice, 2 oz.,	31	„
4. Milk 2 quarts,	232	„
5. Porter, 1 quart,	54	„
6. Tea, 1 pint,	5	„
7. Eggs, 2,	90	„

---

**1670** grs. urea.

**4.—Food per day previous to 29th November, 1860.**

1—7. Same as before, . . .	1670	grs. urea.
8. Boiled cabbage, 2 lbs. .	114	„

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**1784** grs.

**5. Food previous to 6th December, 1860.**

Same as last, with a diminution of the cabbage to 1 lb. daily. This gives for the daily equivalent of urea—

**1727** grs. urea.

**6. Food previous to 13th December, 1860.**

Same as last, with reduction of bread to 1 lb. daily. This gives for daily equivalent of urea—

**1460** grs. urea.



7. *Food previous to 20th December, 1860.*

Same as last; but on 18th December he complained that he had no appetite nor thirst.

8. *Food previous to 3rd January, 1861.*

Meat reduced to  $\frac{1}{2}$  lb. daily; milk reduced to 1 quart; and cabbage altogether omitted. This gives for equivalent of urea—

1. Bread, 1 lb., . . . . .	267	grs. urea.
2. Beef, $\frac{1}{2}$ lb., . . . . .	362	" "
3. Rice, 2 oz., . . . . .	31	" "
4. Milk, 1 quart, . . . . .	116	" "
5. Porter, 1 quart, . . . . .	54	" "
6. Tea, 1 pint, . . . . .	5	" "
7. Eggs, 2, . . . . .	90	" "

---

925 grs. urea.

9. *Food previous to 10th January, 1861.*

Same as last, with omission of quart of milk.

This is equivalent to 809 grs. urea.

## SUGAR EQUIVALENT OF FOOD.

1. *Food previous to 8th November, 1860.*

1. Brown Bread, 2 lbs., . . . . .	9510	grs. sugar.
2. Rice, 2 oz., . . . . .	1243	" "
3. Milk, 2 quarts, . . . . .	708	" "
4. Porter, 1 quart, . . . . .	52	" "

---

11,513 grs. sugar.

2. *Food previous to 13th December, 1860.*

The sugar-forming elements of the food remained unaltered up to the 6th December, when the bread was reduced to 1 lb. daily. Hence—

1. Brown bread, 1 lb., . . . . .	4,755	grs. sugar.
2. Rice, 2 oz., . . . . .	1,243	" "
3. Milk, 2 quarts, . . . . .	708	" "
4. Porter, 1 quart, . . . . .	52	" "

---

6758 grs. sugar

## 3. Food previous to 3rd January, 1861.

1. Brown bread, 1 lb. . . . .	4755	grs. sugar.
2. Rice, 2 oz., . . . . .	1243	"
3. Milk, 1 quart, . . . . .	354	"
4. Porter, 1 quart, . . . . .	52	"

---

6404 grs. sugar.

The following Table gives a summary of the preceding calculations, and a comparison of their results, with the observations on the urine.

TABLE VI.—*Showing the Quantities of Urea and of Sugar ingested and excreted per Day by Owen Murphy, 1860–61.*

Date.	Urea ingested.	Urea excreted.	Sugar ingested.	Sugar excreted.
1860, 8th Nov.	1153 grs.	1559 grs.	11,513 grs.	12,329 grs.
" 15th "	1153 "	1429 "	11,513 "	12,216 "
" 22nd "	1670 "	1420 "	11,518 "	11,250 "
" 29th "	1784 "	1181 "	11,513 "	12,290 "
" 6th Dec.	1727 "	1203 "	11,513 "	9255 "
" 13th "	1460 "	1181 "	6758 "	9545 "
" 20th "	1460 "	976 "	6758 "	6960 "
1861, 3rd Jan.	925 "	849 "	6404 "	7716 "
" 10th "	809 "	840 "	6404 "	6394 "

Reducing the results of the preceding Table to grs. per pound of body-weight, I obtain the two following Tables:—

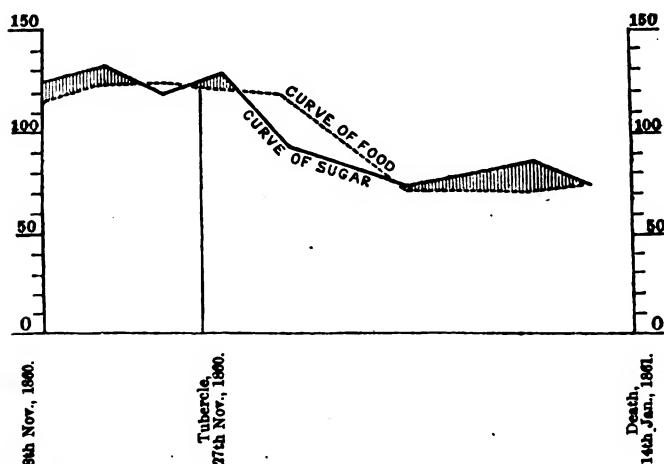
TABLE VII.—*Showing the Relation of the Sugar ingested to the Sugar excreted, per Day, per Pound of Body-weight.*

Date.	Sugar ingested, in grains, per pound of body-weight.	Sugar excreted, in grains, per pound of body-weight.
1860, 8th November,	117 grs.	126 gra.
" 15th "	124 "	132 "
" 22nd "	128 "	120 "
" 29th "	120 "	129 "
" 6th December,	119 "	96 "
" 20th "	72 "	74 "
1861, 3rd January,	70 "	84 "
" 10th "	73 "	73 "

TABLE VIII.—*Showing the Relation of the Urea ingested to the Urea excreted, per Day, per Pound of Body-weight.*

Date.	Urea ingested in grains per pound of body-weight,	Urea passed, per vesicam, in grains per pound of body weight.
1860, 8th November,	11·8 grs.	15·9 grs.
„ 15th „	12·5 „	15·4 „
„ 22nd „	17·8 „	15·2 „
„ 29th „	18·7 „	12·3 „
„ 6th December,	17·9 „	12·4 „
„ 20th „	15·6 „	10·4 „
1861, 3rd January,	10·1 „	9·3 „
„ 10th „	9·2 „	9·6 „

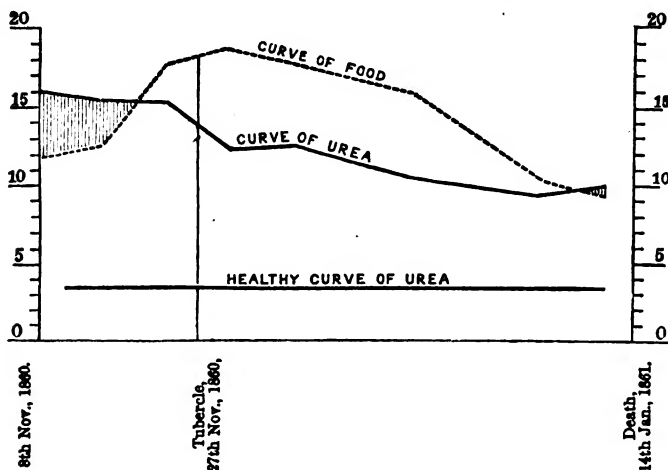
The two preceding Tables are graphically exhibited in the following diagrams:—

No. 1.—*Sugar excreted and ingested.*

In this diagram the horizontal line represents the time from 8th November, 1860, to Murphy's death, 14th January, 1861. The vertical ordinates are drawn to scale to represent grains of sugar per pound weight of body. The full line, called "curve of

sugar," represents the sugar excreted in the urine; and the dotted line, called "curve of food," represents the equivalent of sugar ingested, calculated by converting the starch, &c., into glucose. I have shaded the intervals between the curves, when the sugar excreted exceeded the sugar ingested; during this portion of time the disease must have converted the body-tissues into glucose.

No. 2.—Urea excreted and ingested.



In this diagram the horizontal line is the same as before, and the vertical ordinates, drawn to scale, represent grains of urea per pound weight of body. The full and dotted lines represent the *excreta* and *ingesta*; and I have shaded their intervals, when the urea excreted exceeded the urea ingested; where this shaded space occurs the urea excreted must have been produced partly by the destruction of the proteinic tissues of the body. I have placed on the same diagram the horizontal line of 3.5 grs. urea per lb. of body-weight, in order to show how excessive the excretion of urea is in diabetes mellitus.

The following Table shows the relation of the discharge of sugar and urea to the body-weight:—

TABLE IX.—Ratio of Excretion of Sugar to Excretion of Urea.

Date.	Grains of Sugar per day, per pound, of body-weight.	Grains of urea per day, per pound, of body-weight.	Ratio of Sugar to urea.
1860, 8th Nov.	126 grs. per lb.	15.9 grs. per lb.	7.9
" 15th "	132 "	15.4 "	8.5
" 22nd "	120 "	15.2 "	7.8
" 29th "	129 "	12.3 "	10.4
" 6th Dec.	96 "	12.4 "	7.7
" 20th "	74 "	10.4 "	7.1
1861, 3rd Jan.	84 "	9.3 "	9.0
" 10th "	73 "	9.6 "	7.6
Mean, . . . . .			<b>8.25</b>

The following is Mr. A. W. Foot's account of the case, which was carefully observed by him from day to day:—

"Owen Murphy, aged 20, from Balbriggan, was admitted into the Meath Hospital, October 10th, 1860.

"*Previous History.*—He was in good health fourteen months before admission. In the month of November, 1859, he got a fever, in the course of which he had vomiting, diarrhœa, and an eruption on his body. He was in Ardee Hospital for two weeks; he left this hospital on his convalescence, because he did not get enough to eat. A month after doing so, he began to suffer from excessive thirst; and in two or three weeks more, from great appetite; at the same time he commenced to make water in large quantity, of a "sea-green colour, and having the smell of apples." He had got his livelihood by making bricks in summer, and by out-door farm-work in winter.

"*Condition when admitted.*—Thirst and appetite excessive, passing daily large quantities of pale urine, full of sugar, and of a high specific gravity. Skin dry; never moist. Bowels habitually confined. Muscles thin and weak. Face clay-coloured; and countenance dejected and cold-looking. Sight defective; has been so since commencement of present illness; was good before; the pupils are dilated, and contract sluggishly to light; he sees best at noon; sometime has double vision; sees sparks of light when the eyes are long directed

upon one object. Has a constant heat in his body; so that he throws off his bed-clothes, even on cold nights, to get cool. Cold water relieves this feeling of warmth best. Prepuce long, and cannot be retracted. Pulse rapid (100). Tongue red at the tip, covered towards its back part, with soft brown paste. Suffers often from giddiness.

*Treatment and Progress.* October 14th. Recipe:—Of compound rhubarb pill, a drachm and half; of creasote, ten drops, made into eight pills, one three times daily; and an ounce of lime water in an ounce of milk every third hour.

“Ordered brown bread, and to have it toasted.

“For the first four or five days which he was in hospital he was on milk diet, and consequently said he could not remain, his hunger was so great. He had previously left two other hospitals in Dublin, because he did not get enough to eat. He used to support himself at home by begging among the farmers' houses; and stated that he generally ate 4 or 5 lbs. of meat and as much bread, daily. He expressed a liking for fat meat, and an especial preference for roasted fat. He found his thirst assuaged best by porter; buttermilk, which he sometimes drank to distension, increased his thirst. He was soon ordered full diet: a pint of porter daily, two eggs, an additional quart of milk, and rice for supper instead of stirabout, the usual food.

“October 26th. Ordered a teaspoonful of rennet in water three times a day. This he took till November 22nd.

“Thursday, Nov. 8th. Weighed him; bladder empty. Deducting the weight of his clothes, he weighed seven stone.

“The diet he was taking at this time was the following:—

“*Breakfast.*—Brown bread, 1 lb.; milk, 1 quart; eggs, 2.

“*Dinner.*—Beef, boiled,  $\frac{1}{2}$  lb. four days in week; brown bread, 1 lb.; porter, 1 quart.

“*Supper.*—Rice, 2 oz.; milk, 1 quart; tea, 1 pint.

“The quantity of urine excreted from 9 A. M. on the 9th, to the same hour on the 10th of November, was  $15\frac{1}{2}$  pints; colour, light golden; specific gravity, 1040·2; containing sugar, 12329 grs.; urea, 1559 grs.; the grains of urea per lb. weight being 15·9.

“Thursday, November 15th. His naked weight was 6 st.  $8\frac{1}{2}$  lb., showing a loss of  $5\frac{1}{2}$  lbs. in the preceding week. Professor Haughton accounted for this loss by the fact which he ascertained, that he was excreting more proteinic and farinaceous compounds per urinam than he was supplied with in the ingesta. The boy himself, at this time, said he had a particular craving for meat; his allowance was at once

doubled, and given every day in the week instead of but four times.

"The quantity of urine passed in twenty-four hours, on the 14th and 15th of November, was 18 pints 3 oz., muddy, and fermented vigorously; specific gravity, 1033·8; containing sugar, 12215 grs.; urea, 1429 grs; the grains of urea per lb. weight being 15·4.

"Thursday, Nov. 22nd. The creasote pills omitted. Recipe:—Of extract of aloes and extract of rhubarb, each one drachm; of sulphate of quinine, gum mastich, and pepper, each two grains; make twelve pills; one to be taken daily.

"His naked weight was 6 st. 9½ lbs. The quantity of urine passed in the previous 24 hours was 18 pints; of specific gravity, 1033·8; containing sugar, 11250 grs.; urea, 1420 grs.; the grains of urea per lb. being 15·1. An addition was made to his diet on this day; he was ordered 2 lbs. of fresh cabbage, and the juice of three lemons, daily. His appearance has improved; the clay-colour of his complexion has disappeared.

"Tuesday, Nov. 27th. A bubble was heard in the right infra-clavicular region; the right clavicle is dull on percussion; *bruit de pot felée* very distinct over second rib.

"Thursday, Nov. 29th. His naked weight, 6 st. 11½ lb.; the quantity of urine passed in the previous 24 hours was 15 pints, spec. grav. 1035·0, containing sugar 12,290 grs., urea 1181 grs., the grains per pound being 12·3.

"The diet of the preceding week was—

"*Breakfast*.—Brown bread, 1 lb.; milk, 1 quart; juice of one lemon.

"*Dinner*.—Beef, boiled, 1 lb., every day in week; brown bread, 1 lb.; porter, 1 quart; juice of one lemon.

"*Supper*.—Rice, 2 oz.; 1 quart of milk; juice of one lemon.

"Saturday, December 1st. The infra-clavicular bubble increasing in distinctness; *bruit de pot felée* well marked; is not able to eat more than half his allowance of cabbage; reduced it to 1 lb. per diem; respiration more feeble in right lung posteriorly than in left; both clavicles becoming prominent; has a dry cough, which gives him pain in the right side; heart's action and sounds normal; pulse, in sitting posture, 112.

"Thursday, December 6th. Naked weight, 6st. 12½ lb.; quantity of urine in previous twenty-four hours, 13 pints 15 oz.; specific gravity, 1035·7, containing sugar, 9255 grs.; urea, 1203 grs.; the grains of urea per lb. weight being 12·4.

"Expectoration scanty, thick, yellowish; sputa described as 'sweet, like honey;' does not sleep well; a weakness comes over him at night.

"December 8th. Ordered wine 4 oz; increased next day to 6 oz.

"December 10th. Expectoration profuse, viscid, brownish; pain on coughing; gets no sleep from the perpetual coughing at night; *râles* becoming developed downwards over right side of chest.

"December 13th. Could not be weighed, as he was not able to leave his bed; the urine of 24 hours was 12 pints, spec. grav. 1040·6, containing sugar 9545 grs., urea, 1181 grs.; he can only lie on the right side; has not rested the last two nights from constant coughing; *gargouillement* very decided in right infra-clavicular region; is not able for his full allowance of diet; 1 lb of bread taken off, and the second pound of cabbage; but this latter he took again in a few days, when he was given some butter with it. Recipe:—Of prussic acid, one drop; of solution of muriate of morphia, eight drops; draught to be taken three times daily.

"December 15th. Coughs less, sleeps better, diet now is—

"*Breakfast*.—Brown bread,  $\frac{1}{2}$  lb.; milk, 1 quart; 2 eggs; juice of one lemon.

"*Dinner*.—Boiled beef, 1 lb.; cabbage, 1 lb.; bread,  $\frac{1}{2}$  lb.; porter, 1 quart; wine, 6 oz.; juice of one lemon.

"*Supper*.—Rice, 2 oz.; milk, 1 quart; juice of one lemon.

"December 18th. No appetite or thirst; he drank nothing all last night; made very little water; cough incessant; difficult to expectorate; tongue dry and brown; pulse 106; loud *râles* over right lung anteriorly; respiration feeble posteriorly; ordered flannel vest, jacket, and drawers.

"December 20th. Weighed, naked, 6 st. 8 $\frac{1}{2}$  lb.; quantity of urine in 24 hours, 8 pints 15 oz., spec. grav. 1038·3, containing sugar 6960, urea 976; he cannot lie now off the right side; small streak of florid blood in expectoration last night.

"December 21st. Prepuce red, swelled, fissured; no appetite at all; thirst much less.

"December 31st. Asked for roast meat; allowed  $\frac{1}{2}$  lb. roast mutton daily; vegetables omitted, he could not eat them; a pint of tea instead of the morning milk.

"January 3rd. Weight, naked, 6 st. 7 $\frac{1}{2}$  lb; urine of 24 hours 9 pints 14 oz., spec. grav. 1040·1, containing sugar, 7716 grs.; urea, 849 grs.

"January 8th. Thirst greater, appetite less; gums soft; bowels free; distinct crepitus along front of right lung; sometimes perspires about the head at night; decubitus on the right side.

"January 10th. Weighed 6 st. 3 $\frac{1}{2}$  lb.; urine of 24 hours,



9 pints 10 oz.; specific gravity, 1031·8 containing sugar, 6394 grs.; urea, 840 grs.; in very low state—depressed; no appetite at all; found sugar on a qualitative examination of the expectoration; thirst excessive.

"Saturday, Jan. 12th. Began to vomit; rejected all his food through the day; continued to do so next day and Monday. On Monday, Jan. 14th, tongue dry and brown; pulse, 120, steady; headache, pain in back, depressed, and moaning; decubitus on right side; no food remained on his stomach since Saturday; his prepuce sore; had been several times syringed during the last fortnight; drank quantities of cold water all this day; mind wandered before death; his ravings were about eating; and he spoke of the quantities of fat, "roasted fat," he would eat to-morrow. Died at midnight, Monday night, quietly, without convulsions.

*"Post-mortem nine hours after death.*

"Body warm, soft, very white, shrunken.

"Brain, weight, 49 oz., healthy appearance.

"Pleuræ, no fluid. Right lung adherent to ribs, softened; tubercles thickly disseminated through it. Cavity in upper lobe, size of a walnut. Left lung, no adhesions. Cavity, 3 inches long, 1 inch wide, in upper-third; tubercles through it, more abundant towards apex.

"Heart small.

"Liver large; weight, 61½ oz.; natural colour.

"Right Kidney much larger than left; weight, 7½ oz.

"Left kidney, weight, 6 oz.

"Supra-renal glands well marked.

"Bladder thickened, large; contained about 5 oz. urine. Kept for examination. The stomach was empty, containing only a little dark-coloured fluid. The mucous membrane was thick, soft, and greyish in colour. The seminal vesicles contained abundance of dead spermatozoa."—(*A. W. Foot.*)

I examined the urine found in the bladder after death, and found it to contain mucus, and a small quantity of albumen; separating these, I examined for sugar, and found it to contain 19·29 grs. per thousand. This would only give 1519 grs. in 9 pints; which would indicate a reduction of the sugar-forming faculty to one-fourth of what it had four days previously.

I treated 245·4 grs. of blood from the heart's right ventricle, liver, and general system, by drying and percolation with alcohol (sp. gr. 0·828), and failed to detect the slightest trace of sugar.

The liver weighed 61½ oz. I treated 451 grs. of this liver

with alcohol as before, and failed to find any trace of sugar in it.

In 348.2 grs. of the fatty liver of a phthisical patient, who had died shortly before in the Meath Hospital, I had found, without any difficulty, by percolation with ether and alcohol, sugar enough to saturate 29 measures of the standard copper solution (100 measures = 5 grs. glucose), which corresponds with 4.17 grs. per thousand in the raw liver.

In Cl. Bernard's accounts of his examination of human livers for sugar, I can only find three quantitative results, all of which are considerably greater than the preceding. His results are as follows:—

TABLE X.—*Determination of Sugar in Human Livers, by Claude Bernard.*

Case.	Death.	Name and age.	Weight of liver.	Sugar per thousand.
1	Beheaded.	Ayme, 42.	1.300 kilogr.	17.90.
2	"	Bixner, 47.	1.300 "	Much.
3	"	Lafourcade.	1.175 "	Sugar.
4	"	Viou, 22.	1.200 "	21.42.
5	"	Courtin.	1.175 "	Sugar.
6	Gun-shot wound.	—, 30.	1.575 "	11.00.

If, as some physiologists assert, the kidney is only a filter, and its function be simply to separate the urea and sugar previously formed and circulating in the blood, there ought to be a certain proportion between the dilution of sugar in the blood and in the urine, and we should expect to find sugar in the blood in that proportion as compared with the sugar in the urine.

I have attempted to determine the proportion between the sugar in the blood and in the urine of Owen Murphy in the following manner:—

1. I assume, on the authority of Weber and Schwann, that the blood in the human body is one-eighth the weight of the body.

2. That the left ventricle holds 3 ounces.

3. That Murphy's average pulse was 100 per minute; and that his weight was 96 lbs.

4. I examined, and carefully measured the diameters of the aorta and renal arteries of a subject 82 lbs. weight, and found the following results:—

Diameter of aorta, . . . . .	$1\frac{25}{160}$	of inch.
Diameter of right renal artery, . . .	$\frac{15}{160}$	"
Diameter of left renal artery, . . .	$\frac{25}{160}$	"

I assume, for the moment, that the quantity of blood flowing through the renal artery is to the quantity flowing through the aorta in the proportion of the square of the diameter of the renal artery to the square of the diameter of the aorta. In reality much less blood than is given by this proportion flows through the renal artery, in consequence of friction; but the preceding proportion will give me a maximum result.

I find from the preceding assumptions, that in 64 beats of the heart the blood circulation of the heart was completed; that is, in 0.64 of a minute—and since the area of the aorta is to the joint areas of the renal arteries as 16.214 to 1—very nearly 12 ounces of blood passed through the kidneys in 0.64 of a minute. Increasing this to the length of the day, I obtain finally the result that **1350** pints of blood passed through Murphy's kidneys each day. This result, it must be observed, is a *maximum*; and the less it is, the stronger my argument to be founded on it becomes. The average quantity of urine passed per day by Murphy was **13.37** pints; which is almost exactly the hundredth part of the blood from which it was secreted.

In the urine found in the bladder after death, I detected 19.29 parts of sugar per thousand; this, if it were secreted previously, and pre-existed in the blood, the kidneys only acting as a filter, would correspond to 0.1929 parts of sugar per thousand in the blood. This quantity of sugar could not have escaped the tests I applied. The copper solution I used was of the following strength:—100 measures=5 grs. glucose; and a quantity of oxide of copper less than that contained in one measure may be detected as a precipitate. The following chain shows the number of measures corresponding to the quantity of blood I examined:—

$$\begin{array}{rcl}
 & \swarrow & 245.4 \text{ grs. of Murphy's blood.} \\
 1000 & \swarrow & 0.1929 \text{ grs. glucose by filter-theory.} \\
 & \swarrow & 5 \text{ measures of standard copper solution.} \\
 \hline
 & & 0.946 \text{ measures.}
 \end{array}$$

This quantity of the standard solution, if decomposed by the sugar, would have been detected by me; and as there was

not the slightest trace of oxide of copper precipitate, I believe that the blood, if it contained any sugar at all, did not contain one hundredth part as much as I found in the urine.

But, in reality, the blood should have contained, if the filter-theory be true, more than double the quantity of sugar just calculated; for it is a well known principle in hydraulics, that the discharge of liquids flowing through circular pipes of different bores, varies as the *square root of the fifth power of the diameter*, and not as the *square of the diameter*, which I have assumed in the preceding calculation. If this law be used, the amount of blood passing through the aorta in a given time will be 77·384 times the amount passing through one renal artery; or 38·692 times that passing through the renal arteries conjointly, instead of 16·214 times. From this it follows that only 566 pints of blood passed through Murphy's kidneys per day, instead of 1350 pints. This would give a dilution of only  $\frac{1}{2}$  and instead of  $\frac{1}{100}$ th in estimating the sugar in the blood as compared with that in the urine—this would correspond to two and a quarter measures of the standard copper solution, an amount, which, if reduced by the presence of sugar, would attract the notice of even an inattentive observer.

The quantity of sugar in the urine during life was much greater than that found after death; for example,—on the 29th November, in 24 hours, in 300 oz. of urine there were 12290 grs. sugar, which correspond to 93·6 grs. to 1000 grs. The corresponding quantity of sugar in the blood would be 2·28 grs. to 1000 grs. of blood. I had no opportunity of examining the blood for sugar during life, and cannot, therefore, state what quantity existed in it.

Kane failed to detect sugar in the blood of his case No. 5, although he used a process capable of discovering 5 grs. in 1 lb. of blood; i. e., 0·7143 grs. in 1000. In this case the urine contained 70 parts of sugar in 1000; which, assuming  $\frac{1}{30}$ th as the dilution, would give 1·4 grs. of sugar to 1000 grs. of blood, or double that which he could have easily detected. This experiment of Sir Robert Kane, made without any theoretical view, confirms the fact I have found true in the case of Owen Murphy, in whose blood I should have found sugar, had the filter-theory been true.

The only case in which I can find a quantitative comparison between the sugar in the blood and in the urine, is one recorded by Simon (*Animal Chemistry, &c.*, Sydenham Society Publications, vol. i. p. 327, and vol. ii. p. 295). In this case he found 86·3 parts of sugar in 1000 parts of urine, and 2·5 parts of sugar in 1000 parts of blood [the 2·5 parts not being

altogether pure, but containing extractive matter, and some salts]. This result would certainly fall in with the filter-theory, as it would seem to give a dilution of sugar in the blood not very far from  $\frac{1}{4}$ nd which I have found to be very near the theoretical dilution in the case of Murphy. Many circumstances, however, might combine to render the fraction of dilution greater than  $\frac{1}{4}$ nd, such as the passing of a larger quantity of urine; while, on the other hand, a greater weight of body, and therefore of blood, might tend to diminish this fraction. Without a knowledge of all these circumstances in Simon's and Kane's cases, no exact inference can be deduced from them; but I believe that my own post-mortem experiment on Owen Murphy is sufficient to make out a *prima facie* case against the filter-theory, which ought to be tested by further experiments before it is admitted or rejected.

(To be continued.)

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*De la Syphilisation. État Actuel et Statistique.* Par W. BOECK  
Christiania: H. J. Jensen, 1860. 8vo, pp. 72.

*Syphilisation.* Von DR. FRONMÜLLER, Aertzliches Intelligenz-  
Blatt. March 10, 1860.

*Syphilization.* By DR. FRONMÜLLER, Physician to Christ's  
Hospital at Fürth.

*Syphilisation.* Von DR. LINDWURM, Aertzliches Intelligenz  
Blatt.

*Syphilization.* By DR. LINDWURM, Physician to the Hospital  
at Munich.

THE treatment of secondary syphilis by inoculation with the virus of a primary sore, or syphilization, as it is called, has attracted great attention on the Continent since Auzias-Turenne and Sperino published the results of their experiments some eight or nine years ago. Boeck, of Christiania, whose last pamphlet upon this subject we have been just reading with considerable interest, was one of the first to take it up; and so early as 1853, published a paper upon syphilization, followed by others, on the same subject, in 1854-56-57,—when we inserted in our February number for that year an original communication from Dr. Boeck himself,—in 1859, and finally, this last in 1860, in which, after giving a short account of the mode of using this new method of cure, the cases in which it is most likely to succeed, and those in which it ought not to be adopted, he proceeds to give a detailed account of 300 cases of secondary syphilis treated in the hospital of Christiania, by himself and his colleagues.

Dr. Boeck begins by asserting that syphilization, or inoculation with the secretion from a primary syphilitic sore, effectually and radically cures secondary syphilis—provided that the patient has not been previously treated with mercury or iodide of potassium, although this latter seems less antagonistic than the former to the treatment by syphilization. Of the 300 cases of which he gives a statistical account, only 10 suffered from relapse or were imperfectly cured; and *all* the patients so treated improved in their general health during the treatment. This important fact is also asserted by Dr. Frommüller, of Fürth, who, though not so enthusiastic an upholder of syphilization as Dr. Boeck, yet, after giving this new system a fair trial in his hospital during the year 1859, when he subjected to that treatment forty-four syphilitic patients, declares that all the individuals thus treated, whether cured or not, were invariably improved in their general health and appearance. Dr. Frommüller wisely confesses that “the nature of syphilization and its mode of action is not clearly understood, and its theory yet to be discovered.” But what seems to be established beyond a doubt is, that secondary syphilis is curable by repeated inoculations with the infection taken from a primary sore; and that during the treatment, which sometimes is extended over many months, the most broken-down cachectic patients not only improve in health, but actually gain a robust appearance.

We cannot, therefore, be surprised if, on the strength of his successful treatment of 290 cases, Dr. Boeck exclaims—“This new method (syphilization) has at last realized the hopes of 350 years of experiments and researches for the discovery of a curative agent to replace mercury, which, at the same time that it expelled the disease, would not ruin the constitution of the patient, and be rarely liable to relapse.” It seems to be pretty well ascertained that syphilization should only be practised upon such cases of secondary disease as have not been treated by mercury; and although iodide of potassium does not seem to have as great an antagonising effect as mercury, still those patients who have not been treated with either seem to do best under the new plan. Some German physicians, who have largely practised this treatment by inoculations, seem to think that tertiary symptoms are less amenable to it than secondary ones, and that with pregnant women syphilization is much less likely to succeed. Different views as to the mode of action of this repeated inoculation have been put forth in Germany. Many are of opinion that the inoculation of any matter whatsoever which could produce a pus-secreting

ulcer, would have as good an effect; but we feel with Dr. Fronmüller, of Furth, that we have yet to discover the theory of the peculiar action of syphilization, and that more experiments are required before we can, with any degree of certainty, lay down as law that this plan of treatment is, in reality, a super-saturating of the constitution with syphilitic poison, or only a clearing out of the morbid element by innumerable issues produced artificially all over the body. Two facts, however, have been pretty well ascertained, if we are to give credit to the numerous accounts published on the Continent—1st, that syphilization *cures secondary syphilis*; and 2nd, that it is *no prophylactic against re-infection*, as had been hoped by Auzias, Sperino, and many others, and is still believed by a few. Dr. Boeck, of Christiania, has given a list of 300 names of individuals on whom he practised syphilization, together with a description of those several cases and the results of his treatment, which proved completely successful in 290; and we shall extract from his pamphlet an account of his mode of proceeding.

He commences by cautioning physicians against employing syphilization in cases which have been previously treated by mercury; for in them the treatment by inoculation seldom runs an even course; and although eventually it will cure, yet he prefers that this treatment should be adopted solely in those cases which as yet have had no mercury. The same objection arises in cases previously treated with iodide of potassium, though in a less degree. The sooner the inoculations are practised after the appearance of the secondary symptoms, the more rapid will the cure be. The mode in which the inoculations are practised is as follows:—Having obtained some infection from a genuine syphilitic chancre, Dr. Boeck introduces it under the skin as in vaccination, selecting the sides of the thorax for the first inoculations, of which he makes three on each side. After three days he makes three fresh inoculations on each side of the thorax, *with pus taken from pustules resulting from the first inoculation*; and in three days more, he makes three fresh inoculations on each side of the thorax, with pus obtained from pustules resulting from the *last* inoculation; and so on he inoculates every three days in the same region, each time with pus obtained from the *last pustules*, until inoculation ceases to produce any result whatever. He now proceeds to inoculate over the two arms, continuing to do so as he had done over the sides of the thorax, until a point is reached where the poison introduced ceases to have any effect. He then passes to inoculate over the thighs, after the same man-



ner as had been practised over the thorax and arms; and, finally, he inoculates the patient over again with fresh infection obtained from other patients, until no effect whatever is obtained. In broken-down cachectic individuals, these inoculations have at times been followed by very ugly-looking ulcers, which have even in some instances taken on a phagedenic action. Dr. Boeck is evidently alive to this danger, and probably experienced it himself; because, though he does not mention it, he says, "When I possess some virus which has passed already through several individuals, I select it to commence with; but as the process of syphilization progresses, the more active infection may be employed without fear of its then producing phagedæna." Dr. Boeck places great stress on the locality to be selected for the commencement of these inoculations; and he asserts that the syphilitic virus has different degrees of intensity when applied to the different regions of the body, being most violent in its effects when applied to the thighs, and he therefore always leaves those for the last. Care must be taken, also, not to practise the inoculations too near to one another, lest they should become confluent, and by running into each other form ulcers of such size as to occasion risk of erysipelas and inflammation of the lymphatics, not to mention the risk of unsightly cicatrices. Although he generally practises three inoculations on each side every third day, yet he would be quite contented with one, if he were sure that it would take, as the safety of the patient does not depend upon the number of inoculations, but upon the simple fact that all inoculations of syphilitic poison cease to produce any effect. Three days are allowed to elapse between each inoculation, because less would be insufficient to produce pustules giving pus; and in some cases even more than three days' interval may be requisite. Often, with infants suffering from hereditary syphilis, the first inoculations will fail. In these cases the rule is to continue inoculating every second day until the virus produces a pustule, when it often happens that all the previous inoculations start into action. The same peculiarity is sometimes remarked with patients suffering from rupia, or some very extensive papulo-squamous eruptions. As to the local treatment, it is the same as in any other case of superficial ulcerations; and any mild dressing may be adopted, as circumstances may direct. With children it may be often necessary to administer some mild sedatives, to allay the irritation caused by so many little sores. In both young and old the diet should be abundant and nutritious, at the same time that alcoholic drinks should be avoided.

These are Dr. Boeck's rules and directions; and are followed by tables, wherein are detailed 300 cases treated by him in the hospital of Christiania. Are they to be depended on? We think they can to a certain point, if not in their totality; for we have them corroborated by no less an authority than Dr. Lindwurm, of Munich, who places special value on the writings of Boeck. He states, as we read in the "*Aertztliches-Intelligenz-Blatt*," "Infants, adults, and old people have been treated by this method with success." The time required to effect a cure will vary according to the nature of the disease, its severity, its length of duration, the constitution of the patient, and the previous treatment. Lindwurm believes, from his experience, that from ten to twelve inoculations at a time, repeated every three or four days, will best lead to a cure. He does not, however, adopt this new method unreservedly; and he fully admits that it is an unpleasant, troublesome, disgusting, and by no means painless, mode of treatment; that it requires a long time; and finally, according to him, affords no certainty of cure. Its greatest value, in Lindwurm's eyes, consists in its always improving the patient's health, let that be ever so much broken down; "so that weakly, worn-out, patients become strong and robust under this process; and even weakly infants, affected with hereditary syphilis, become healthy and strong." But notwithstanding all this, Lindwurm grants to syphilization only a secondary place in the therapeutics of syphilis, still holding that in iodine and mercury we possess simpler and surer remedies, and which, with care and watching, will produce few disagreeable after consequences. But in such cases as broken-down cachectic constitutions, in which mercury would be inadmissible, Lindwurm would recommend a trial of syphilization. But by this process of inoculation is the patient really syphilized; is he, in other words, saturated with syphilitic poison, and thus rendered unsusceptible of taking in any more; or is he cured by a process of derivation brought about by the suppuration of some hundreds of small secreting ulcers? Boeck says, "In order to prove that syphilization is nothing else but a derivative treatment, its opponents return to the old doctrine of the duality of syphilitic virus. Simple chancre does not infect the constitution; and it is always with pus obtained from one of these that the inoculations are practised, simply because a hardened chancre (Hunterian chancre) does not, as they say, yield inoculating pus. The subject, therefore, who has been syphilized has only suffered from a vast number of simple ulcers, and the pretended syphilization is only a monstrous illusion—a complete error!

And are the hundreds who have been thus cured so many illusions also? Surely, if one had spoken thus ten years ago, and before experiments had been made on man, all those remarks might have been looked upon as very sage, and rightly deduced; but when now-a-days we hear long arguments against syphilization, based upon such grounds, one finds it difficult to listen to them seriously. Even supposing it is by derivation that the syphilitic symptoms disappear, is the fact of syphilization lessened thereby? The name—syphilization—might, perhaps, be badly selected, but the results would remain the same; and the idea of treatment by syphilization would have taught us that syphilis could be cured by derivation.

“But I may be told, perhaps, that there is no necessity in rejoicing over the discovery of the cure of syphilis by derivation, when we have in mercury a better remedy. Well, I can understand that syphilization be abandoned, if it is thought that mercury is both a sure and safe remedy—that would be rational; but if, on the other hand, it is admitted that mercury is not free from danger, I cannot comprehend how syphilization should be rejected because it is nothing else than *derivation*.”

In looking over the statistical tables which form the bulk of the pamphlet, we are struck by the high mortality of infants suffering from hereditary syphilis, who were subjected to this treatment. Out of sixteen thus treated, fourteen died shortly after its commencement, viz., one in two, one in nine, and the remainder in a period averaging between fourteen and thirty-five days. The average duration of the treatment of all the 300 cases was 144 days.

To us the most surprising fact connected with this new process, is the constant improvement of health during this continued inoculation with syphilitic poison. We say fact, because we find Dr. Boeck's statement to that effect nowhere contradicted; and, on the contrary, confirmed by Lindwurm, whose assertions in this matter we are compelled to receive with all the respect due to his well-established reputation. We cannot refrain quoting a passage from Dr. Boeck's essay, which appeared in the February number of this Journal for 1857: “It is a great physiological fact, that the syphilitic virus, by continued inoculation, annihilates itself; for this leads us to the result, that by syphilization we have entered upon a law of nature which can scarcely exist of the syphilitic poison alone. Other animal poisons probably must obey the same law.” Dr. Boeck tells us, that “in some instances the

inoculated person becomes proof to one sort of virus; I then take the matter for inoculation from another, preferring a case which has had a different origin; this then proves effectual. But sometimes they become proof to this also, and then I seek for a third source; and thus I go on as long as any matter at all will operate." Dr. Boeck has a wholesome dread of mercury, without a corresponding faith in its curative powers. He says—"I do not think that any physician in Scandinavia would say, with M. Ricord, that in from four to six weeks he heals every constitutional syphilis." Now, we have to make great allowance for Dr. Boeck's feelings on the subject of mercury; for its action on the human economy is very different in high Northern latitudes than in more genial climates; and those who have only witnessed its action in these countries can form no idea of the irregular course it often pursues in Northern countries, and the serious after consequences it produces. Although syphilization seems so powerful in subduing secondary symptoms, it appears less efficacious in tertiaries; and Boeck states, that "in affections of the osseous system, syphilization hardly ever seems to produce any effect."

Dr. Fronmüller, physician to the Christlichen Hospital of Fürth, gives an interesting account of the treatment of syphilis by syphilization; and in the wards of his hospital, both men and women affected with chancres, condylomata, leucorrhœa, syphilitic eruptions, and buboes, were subjected to this treatment. He seems to have been highly successful, as the following cases, taken at random out of a long list of similar cases, will show:—"The first experiment I made was with a woman aged 32. She was admitted into hospital in April, 1859, suffering from ulcerations, with everted edges on the inside of her mouth and tongue; and also on the pudenda. She had considerable vaginal discharge. The neck of the uterus was, on examination, found to be small and pale; and the os was surrounded by numerous erosions, which bled at the slightest touch. Her neck and chest, and her back, even down to her buttocks, were covered with numerous copper-coloured patches. The patient seemed to be of good constitution. On the 2nd of May, I commenced the treatment by inoculating on the inside of each thigh with pus obtained from one of the chancres in the vulva. So soon as the following day, the spots which had been inoculated had become studded with pustules, each surrounded by a red areola. On the 4th of May, these pustules had acquired a diameter of a quarter of an inch; and six more inoculations were practised with the same infection as before, in the immediate neighbourhood of the first inocula-

tions. On the following day, the copper-coloured eruption was remarked to be paler, and the ulcers within the mouth were diminished in size. On the 6th of May, the pustules resulting from the inoculation of the 4th were as large as those of the 2nd. Six inoculations were made now on the thighs, with fresh matter obtained from the ulcers in the pudenda; and two inoculations were practised on the top of each arm, with pus from the ulcers of the mouth. By the 7th, the eruption had nearly faded away, whilst there was an abundant crop of pustules resulting from the inoculation of the 6th; and also some pustules appeared both on the chest and back, independent of any inoculation. On the 8th of May, six fresh inoculations were practised on the upper arm. By this time the ulcer on the left labium was healed. By the 10th, the pustules which had independently made their appearance over the chest and back were beginning to dry up; and the ulcer in the mouth had diminished in size, as also the erosions around the os uteri. On the 12th of May, the eruption was no longer visible. On that day two more inoculations were practised on the breast, with matter obtained from the ulcer of the mouth, which by the following day were surrounded with small pustules. On the 15th, fresh inoculations were made, but without success. The vulva was now nearly free from disease. The pustules of the first inoculations had dried up and fallen off, without leaving any scar. On the 2nd of June, the patient was discharged, cured."

"On the 12th of July, a patient aged 18, was admitted into Hospital suffering from œdema of the labia, chancres, vaginal discharge, and condylomata. After sixty inoculations, there remained only a slight discharge, and the condylomata;—after the 122d inoculation, the patient was discharged, cured."

"On the 16th of August, E. W., aged 29, was admitted into Hospital, suffering from redness of the pharynx, numerous ulcers of the labia, and condylomata round the anus. She was treated by syphilization, and from 4 to 8 inoculations were immediately practised on the inside of her thighs with matter taken from the ulcers. The inoculations were afterwards practised over the arms, and lastly over the sides of the chest, and below the breasts. Ulcerations as large as half an inch in diameter formed themselves on the inside of the thighs, and pustules soon made their appearance on the arms and sides, which by degrees acquired a diameter of half an inch, and in some spots became confluent. The progress of the treatment was most satisfactory: as the inoculations increased in number, so the ulcerations of the genitals first disappeared, and the condylomata diminished

in size; after the 80th inoculation, the ulcers were perfectly healed—all redness in the pharynx had disappeared, the vaginal discharge had ceased; and although the condylomata still remained, yet they were diminished to one half their original size. From this time to the 26th of September, about 70 inoculations more were practised, the last of which produced no effect. By the 30th of September, even the refractory condylomata disappeared. The patient was kept in Hospital till the 4th of October, in order to heal completely all the ulcers resulting from the latter inoculations, of which she had 150 in all, the 12 last of which failed to produce any result." This case relapsed, and it was the only one which did so in Dr. Fronmüller's practice. He says, "This patient, who had been dismissed on the 4th, returned for admission on the 16th. Ulcers had broken out over the spots where the inoculations had been practised, and the vaginal discharge, together with the condylomata, had also reappeared. Whether the relapse in this woman, who was five months pregnant, was due to a fresh infection, or whether it was the result of an imperfect cure, cannot with certainty be laid down." He does not state how this patient was eventually cured.

Dr. Fronmüller has evidently been made a convert to the new system, as can be clearly seen by his accounts of the many cases he has treated in the Hospital of Fürth, and from which we selected the foregoing ones. Stenberg also gives a confirming account, stating that by repeated inoculations of an individual with syphilitic pus, the chancreous sores which are produced gradually diminish in size, until at last, pus which shows itself fully capable of inoculating others, comes to have no effect upon the recipient.

Lindwurm, on the other hand, is only half converted to the new doctrine. He has succeeded in obtaining perfect cures, as will appear by the following case, which we give in his own words; but he has evidently some doubts in his mind, as will appear further:—"I admitted into my clinical wards, on the 23rd of last May (1859), a robust man, aged 35, suffering from extensive excoriation of the glans penis, with considerable induration of the urethral orifice, and numerous warty growths and condylomata, both on the penis and scrotum; he was also covered from head to foot with eruptions of various kinds, copper-coloured, papulous, and pustulous. Some of the pustules were dried up, and covered with a thick brown crust, especially about the nose, mouth, and chin. The fauces were free from disease. In addition to all this, the patient suffered also from chronic eczema, which he had for eight years.

Two hundred inoculations were practised upon this man, commencing first over the thorax, then over the arms, and lastly on the thighs, in which locality the ulcerations were deepest, and took longest to heal. Already, on the thirtieth inoculation, the patient was decidedly improving, and at the end of three months he was completely cured, without reaching the point of absolute immunity to all inoculation. He has remained in excellent health to this hour (March, 1860), and has even lost his old enemy the eczema."

Lindwurm was struck with the fact that his patient was completely cured of his syphilis without having become unsusceptible to syphilitic inoculation; and he determined upon repeating the experiments of Hjort, who, contrary to the opinion of Auzias-Turenne, Sperino, and others, who assume this treatment to depend upon a saturation of the body with syphilitic poison, believes it to be simply a derivative mode of cure, in which the syphilitic poison, *as such*, is quite unessential, and acts only as a simple irritant. Hjort produced a crop of pustules over the body by means of tartar-emetic pomatum, and obtained very favourable results. Cullerier and others, acting on the same principle, tried oft-repeated blisters of small size; every day from four to six small blisters were applied over different parts of the body, and by this means not only maculæ but papular eruptions, and even condylomata, quickly disappeared; squamous eruptions and indurations of the glands, on the other hand, resisted this treatment. Lindwurm tried inunctions with tartar-emetic emetic ointment by itself in fourteen cases; in some, the results were most surprisingly favourable; in some, less so; and in some, no result whatever was obtained. The following cases, related by him to the Pathological Society of Munich, will be read with interest, both from the form of the disease, and the rapidity of the cure:—

"A childswmaid, aged 17, came, on the 24th of August, 1859, to seek admission into hospital for a pustulous eruption covering her forehead, neck, and arms; the glands of her neck were considerably enlarged, and she had a whitish and somewhat elevated ulcer on the inside of her upper lip. No appearance of disease could be detected in or about the pudenda, and her hymen was unruptured; in all probability, she had been infected by the mouth. The treatment consisted in daily rubbings with tartar-emetic ointment over the upper part of each thigh, until a copious pustular eruption was established. So soon as the first crop was dried up, a fresh inunction was made, until another crop of pustules appeared; and

so soon as these had dried up in their turn, a third inunction was practised. By the time this last eruption was dried up, all the syphilitic symptoms had greatly diminished; the glands were no longer swollen and hard, the throat was well, and only a few scarcely perceptible scars remained to point out the spots where the original pustules had existed. By the 8th of September even the ulcer in the mouth was healed, when she left the hospital perfectly cured.

"A housemaid, aged 19, suffering from superficial ulceration of the fauces, violent pains of one side of her head, as also from enlarged glands of the neck, with vaginal discharge, ulcerations of the os uteri, and condylomata, was admitted into hospital, and treated exactly as the previous case. Four separate inunctions were made on her groins, and also in three different parts of her thighs; and in seven weeks she was discharged, perfectly cured.

"The most surprising, however, of all the cures was one I saw in a man, aged 27, who was suffering severely from general syphilis. His breast, back, shoulders, and legs were thickly covered by numerous copper-coloured blotches, mixed with small indurated ulcers, while his genitals were covered with warty excrescences, and his anus surrounded by condylomata. He was treated exactly as the former case. After ten days a very decided improvement could already be seen; and in thirty-seven days, after employing four ounces of tartar-emetic ointment, he was dismissed, cured."

In cases of genuine indurated chancres (Hunterian chancres), as also in cases of psoriasis syphilitica, this treatment seemed to have no effect. Two females, affected with general syphilis, treated in the same manner with tartar-emetic ointment, were sent out of hospital, apparently cured; but in eight or ten weeks they came for readmission, the disease having returned in precisely the same form as that for which they had been treated. This time a modification of the previous treatment was adopted; and instead of inunctions with tartar-emetic ointment, issues were practised in the upper part of each thigh and arm, of the size of about a two-shilling piece, by means of Vienna paste (*potassa cum calce*). When the eschars had fallen off, there remained behind four deep pus-secreting ulcers, which were left to heal of themselves, and no other treatment was adopted beyond rest and good diet. They left the hospital perfectly cured.

"Now, if we compare the two modes of treatment, viz., that by syphilization, and that by derivation with tartar-emetic ointment, we find a similar result, with this difference only,



that the cure by tartar-emetic seems to be more rapid than that by syphilization."

Lindwurm, in conclusion, says that, apparently any derivative treatment, whatever the irritant adopted may be, will produce the same effect as by syphilization; but a much more extended series of experiments will be required, before any one shall be in a position to speak with certainty on this subject; and in the mean while, he considers that we possess in mercury and iodine surer and more appropriate remedies, which, according to him, ought not to be superseded by either syphilization or derivation.

Lindwurm is evidently half-converted by this new doctrine; but before he forsakes his old and endeared friends mercury and iodine, he wishes most properly to have still more evidence in favour of the new system. Two facts, however, seem to be well established up to this, viz., that secondary syphilis can be cured by inoculations with the pus from a primary sore, and that patients so treated invariably grow strong and healthy under this process. Now, we cannot pass any judgment on this subject; we have no personal experience of it; but we have experience of mercury, and we cannot, assuredly, say as much for it.

We shall look forward with great interest for fresh information upon this subject; and should it eventually be proved that syphilis really antagonizes syphilis, as Dr. Boeck is persuaded of, we should be inclined to believe, with him, that this new law in physiology would hardly be limited to one animal poison, but most likely be a fundamental law, ruling both over the syphilitic and other organic poisons also.

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*The Forms, Complications, Causes, Prevention, and Treatment of Consumption and Bronchitis, comprising also the Causes and Prevention of Scrofula.* By JAMES COPLAND, M.D., F.R.S., F.R.C.P., &c., &c. London: Longman & Co. 1861. 8vo., pp. 440.

WHAT are the "calamities of authors" to the calamities of those overworked beasts of burden, the reviewers? How it comes to pass that the literary camel gets through the work he does, and, without repining, carries what authors remorselessly put upon him, it would tax the most learned in zoology to explain. As for our camel, the faithful animal whose load is, at this moment, strapped upon our shoulders, can it be wondered at if his back has at length yielded to

the last straw? He never quite recovered the strain brought about by the last volume of the Cyclopædia of Anatomy. He was observed to despond as the Sydenham Society got upon him. The bantlings of Drs. Routh and Richardson he did not seem so much to mind; and, as a faithful camel, he stood up even under Mr. Cordy Jeaffreson. But now! Camels, it is true, were made to bear, but surely not such an incubus as lies before us. Such heaps of quartos, octavos, and duodecimos. Such piles of Pamphlets and Journals; of Essays and Remarks; of Observations, Suggestions, and Dissertations; pyramids of Replies and Disquisitions; of Elements; of Vestiges; of Transactions and Researches! The Human Skeleton from Cambridge. The Button Suture from New Orleans. Dr. Heale's Vital Causes, and the Constipated Bowels of Dr. Birch. Successful treatments of every infirmity that Adam the wicked bequeathed to his progeny. To wit—of Congestion of the Windpipe, from Hastings (with remarks on the sea-air); of something, and something else, without copaiba (for which may Mr. Weeden Cooke be made M.R.I.A.). Of Diabetes, by Dr. Camps. Is this author the eloquent and prolific speaker at the London Medical Society? Of Hemorrhoids by Nitric Acid, by Mr. Smith. Of Hemorrhoids without Nitric Acid, by Mr. Jones. Then images on all sorts of pedestals. Dr. Barker on the Tadpole-slippery? Dr. Beale, on the Urine;—where we are sorry to leave him. Dr. Combe, for the hundredth time, on the Management of Infancy;—a most difficult business, according to our experience. One inhuman medical Pickford, from Copenhagen, puts upon him "Statistike Undersøgelser af lægeriders Kåbeligkt Inhold." A second foreign gentleman, "Den Kehlhoff Raehenspiegel;" and a third, "Islenzka Homöopathien og Nordlenzku Prestarnin. Ritad af Dr. Hjaltalin. Reykjavik!" But the actual disgrace of breaking the camel's back was reserved, we regret to say, for an Englishman. Dr. Copland had found camels to carry his trunks. This one should bear his parcels; and—we put it to the Profession—is not this venerable camel-driver amenable to the Humane Society?

Not that we are unmindful that men of genius are privileged to boast a characteristic. And if that characteristic crops out to our damage, yet we will not forget that Dr. Copland is one of the lights—one of those few to whom it is given to stand on the platform in the sight of men. But then the Doctor's is such an awkward characteristic. Shakspeare had his many-sidedness; Keats, his sensuousness; David Hume was essentially emphatic; Alcibiades was adaptive—and Copland is

decidedly wordy. Now, though wordiness is a bore, yet we will remember the platform. Standing on that platform, we respect the *man*. If he have but glanced at one in a thousand of the authors he quotes in his Dictionary, he is an extraordinary man. If he remember but a small fraction of what he himself has written, he is a truly wonderful man. Old Birch, and Bayle the copious, are nothing to him. He must write swifter than Dumas, and know more languages than Mezzofanti. He is as insusceptible to fatigue as Hannibal or Lord George Bentinck. He is like—but with what can he be compared? He is like a bladder, full to bursting. Prick him on any one side, and out gushes a torrent of learning. Or he may be likened to the Mammoth Tree at the Crystal Palace, which you have to look at in sections, and wonder how it was made.

Yet it is our duty not to permit our judgment to be blinded by our admiration—however great this latter may be; and when, after we have staggered under four closely-printed volumes on every subject, known or unknown, that ever was or will be, could be, or should be, written, on medicine theoretic and practical; on pathology and the nature of disease; on the cure of disease; on morbid structure; on disorders peculiar to climates, to periods, and to sexes: not forgetting principles; a copious Bibliography; innumerable references, and a choice list of mottoes;—our author puts upon our back another parcel, having a title as long as a book need be—"The forms, complications, causes, prevention, and treatment of Consumption and Bronchitis; comprising also the causes and prevention of Scrofula!" is it to be wondered at if we feel that endurance has its limits, and that the time for remonstrance has at length arrived?

In our humble opinion, then, such a book was not wanted by any individual in the three kingdoms. It was not wanted, for it has no contents to recommend it; and, if it had, they have already been given to the world. *Un amour rechauffé ne vaut rien*; nor can a treatise on Consumption, founded on opinions and views of its pathology, diagnosis, and treatment held twenty years back, be subservient to any more useful purposes than the filling-up of a book-shelf, or enabling our eldest daughter to sit up to the piano. Such a book resembles a bit of amber in which is to be discerned an antediluvian fly. The contents are wonderful, because they are so very old. This work is of no use to the public, for it is too prosy. It is of no use to the physician; for, if he wants new views, he must seek elsewhere; and, if he wants old ones, he can get them in the museum. It is worse than useless to the student; for it gives

him false ideas of practical physic, and tends to lower in his estimation those principles, without which he cannot hope to become a successful practitioner.

Judging from the most salient feature of the work before us, it would appear, that in sad contrast to history and the exact sciences, medicine imbues with the most rampant egotism those who devote themselves to its pursuit. It would seem that, in our profession, it is impossible, as we get older, to avoid resorting in argument to that vice of depreciation—that supercilious contempt of youth and its efforts, which, in the absence of better weapons, patriarchs do so delight to wield. When Samson pulled down the pillars, we doubt not several aged Philistines went about, saying, “It’s so like him;” and “what can you expect from a strong young man?” The strength and vigour of the disciples of Laennec and Louis must, we grant, be very distasteful to the old school of physic. It must be hard, we allow, to be instructed at the bedside by a young man up in cell therapeutics, the duality of a miasm, and the function of the posterior columns; but if the modern schools of pathology are to be upset, it will not be by the shoulder-shrugging and head-shaking, so much the fashion at the present day. The only parallel we can hit upon to the unseemly *animus* of the old against the young school, is in the wholesome example set by the heads of the sister profession towards those writers who give a new version of the Mosaic Cosmogony. If we had a Jowett, Dr. Copland would call him a Lapsarian; and if we had a Baden Powell, he would call him a heretic. Nor can the old school see anything unfair or unphilosophical in this. To them, the principles they imbibed young, are principles for all time. The mistress espoused in the heyday of youth is beautiful still to them; and it is impossible to teach them that she is partial to younger votaries. Novelty, as such, they detest; for they believe themselves to have been educated precisely at the right time, as far as the possible perfection of medicine is concerned. They are, consequently, sorry to say there is a rash way of dealing with disease. Successful treatment depends on a correct estimate of “condition of vascular action,” and of the “circulating fluids,” of which young physic is lamentably ignorant; and cannot be hoped for without a due reference to “the conditions of vital force, of vital energy, power, or endowment,” which it is denied to any man under sixty to appreciate in the slightest degree. The young physician, forsooth, has his mind “pre-occupied by the recognition of changes of structure and of palpable organic alterations,” which shut out from his apprehen-

sion those principles which guided old physic to "practical results."

These tinkling phrases, "conditions of vascular action and of the circulating fluids;" "estimation of those conditions of vital force and of vascular action" recur from time to time, like the sound of the gong, with which, as they march to battle, the Chinese frighten barbarians. We should have thought the superior claims to medical prescience implied in these phrases had been sufficiently paraded in the preface; but, on dipping into the book, we hear the gong again at page 2.

"I can fully corroborate the inference drawn by my friend, Dr. M'Cormac, of Belfast, in his unpretending, but very able and learned, treatise on consumption, 'that the existence of tubercles may be inferred from the general aspect and condition of the patient,' before any physical examination is instituted—'before the patient has spoken a word, or emitted a sound.' The earlier, and *many of the more recent, cultivators of auscultation*, overlooked the fact that solitary or scattered tubercles in the lungs do not necessarily affect the sounds in the lungs—that tubercles do not always furnish a physical sign—in many even when the constitutional and physiological symptoms evinced the early stage of phthisis—the stage in which the recognition of the malady is most important, and in which a true estimate of these symptoms should be made and chiefly confided in: they relied *too confidently on the physical signs, and neglected the physiological and rational symptoms.*"

Is it not too bad that before we have recovered from this castigation, we should be doubled up again at p. 4? And our author, we think, in talking as follows, is somewhat rash in a commercial point of view. We are sure the old school think far too highly of themselves to dip into any works on medicine. The prospects, consequently, of a second edition depend very materially on Young Physic; but it is scarcely probable they will strive very hard to perpetuate so unflattering a portrait of them as is here drawn by the Doctor:—

"The early aberrations from health which indicate the commencement of tubercular phthisis, have been, during the greater part of the period of which of my experience is cognizant, either imperfectly estimated or overlooked; whilst attention has been, and still is, directed alone to that which, although fully deserving a due portion of attention, should not receive an undivided investigation—to *physical diagnosis* in the several forms and methods, in which it has recently been *paraded, over-estimated, and lauded*. Owing to this one-sided study, to the fallacies inseparable from its nature, and to those which arise from varying conditions of vital influence and action, from different states of secretion and excretion, from

numerous disturbing causes appearing contingently, and from *habits of dogmatizing*, with the view of exhibiting a precision of acquirement and knowledge beyond what has been previously reached, the cultivation, if not the advancement of physical diagnosis, to the neglect of the intimate observation of constitutional and physiological changes, has been generally attempted. Manipulations which strike the senses of the attendants, and more than one sense of the patient,—examinations which may be seen, felt, and talked about, have a much more impressive and lasting influence upon both patient and spectators, than the close observation of symptoms, and the pertinent inquiries of the *profound and comprehensive thinker*. The former are lights which the possessor places upon an eminence for his own advantages; the latter are intended entirely to benefit the person for whose safety they are employed.

“Auscultation, which is of great service in the progress of phthisis, is much less advantageously employed at the commencement, and even during the early stage of the malady. Too great dependence upon, and a too *ad captandum* parade of, this mode of diagnosis, sometimes even with the fussiness and the flourishes of vulgar craft, have tended to the neglect of those states of vital manifestation, of disordered functions, and of vascular action, which, whilst they indicate incipient or early pulmonary disease, also characterise its forms, and point to the changes in which these forms originate, and on which they continue more or less to depend.”

This is pretty language; perhaps a trifle too highly spiced. But how pleasant, is it not, to find such genuine philanthropy united to so much modesty! Sweet subject for contemplation—The *profound and comprehensive thinker* employing his lights, not for his “*own advantages*,” but entirely “*to benefit the person for whose safety they are employed!*” For our own part, we feel guilty from head to foot; and thank our stars that so much forbearance towards us yet lingers, where the monopoly of diagnosis and power of treating disease are alone permitted to dwell. On the whole, however, and though we feel it to be a great honour that we should have our nose pulled by a Clutterbuckian, we would crave a little mercy in the next issue. Fancy being growled at through 450 pages. Adieu, microscope! Farewell, test-tube! Deceptive *ignes fatui*, see what you have brought us to! Had we but stuck to the old lights, what “practical results” we might by this time have been able to boast of!

We wonder if, in return for so much courtesy, we shall be thought wanting in all rules of *bienseance* and professional boredom, if we venture to probe the charges, and fathom the depths of the “profound and comprehensive thinker,” who, if unlike

noble Festus, is not similarly affected by much learning, at least, in envy towards his younger relatives, successfully emulates the sisters of Cinderella.

The question, after all, lies in a nutshell. Does the pursuit of physical diagnosis, the application of the microscope to pathology and medicine, the rendering chemistry auxiliary to therapeutics, invalidate the pursuit of medicine as an art, render nugatory the cultivation of the senses, and, in fact, by transforming the physician into a mere reflector of physical impressions, disqualify him as a theorist for the discharge of clinical duties, and retard the progress of medicine as a science of observation? Is it true that physical and subjective medicine are opposed—that the successful treatment of disease can be based solely upon an apprehension of symptoms, which cannot be rightly interpreted by any one grounded as a chemist, proficient as a microscopist, or addicted to the physical method? Cannot a practitioner be at the same time “up” in the several kinds of fermentation, and able to diagnose enteric from typhus? Is it out of the question to suppose that a medical man can detect incipient cirrhosis, if he happen to be able to formulate the saccharine series? Does a knowledge of cytogenesis disqualify for discriminating carcinoma from fibrous tumour; or the ability to recognize a sarcina, make it impossible to distinguish apoplexy from meningitis?

This is not unfairly stated. Either the study of symptoms is everything in medical education, the records of mere observation and experience inefficient for clinical purposes; or far too lavishly have the favours of the public been showered upon the Graveses, the Stokeses, the Brights, the Bowmans, the Walshes, the Williamses, the Johnsons, and the Todds. These are they who personify the triumph of the inductive school. They are evidences, not so much of individual success, as of the invincibility of the principles which guide them, of the principles they teach, of the principles enlightened minds unhesitatingly acknowledge—of those principles, in short, which have lifted medicine, hesitating, dogmatic, blundering, and opinionative, to the rank of an exact science. If it is to them that, in pathology, we are indebted for our knowledge of the nature of tumours, the occurrence of fatty degeneration, the conversion to amyloid, the regeneration of bone, the cycle of inflammation, the results of elimination on secreting texture, the process of albuminoid softening;—if, in physiology, for what we know of the functions of nerve centres, the nutrition of the ultimate cell, the generation of animal heat, the formation of sugar, the production of pigment,

the secretion of the pancreas, the influence of the sympathetic upon temperature, the periodic evolution of the human germ, and the true nature of epilepsy—if, in clinical medicine, for the differential diagnosis of cardiac and renal dropsies, of true and false chancre, of cerebral decay and extravasation, of diseases of retina and choroid, and of gouty and simple inflammation—shall we say that their work is valueless, and that they are successful under false pretences?

If Dr. Copland can abstract himself but for one moment from symptomatology, the sole object of his devotion, he will be led, we are confident, to acknowledge that beauties are to be found elsewhere. Dr. Copland is perfectly aware, for example, if he will refer to his own Dictionary, that the urine frequently deposits sediments having general characters almost identical. Should he, however, attempt to discriminate between them by the aid of his unassisted senses, however acute they may be by long cultivation, he will in the majority of instances find himself considerably at fault. He might touch them, smell them, and taste them, and the sole result would be that he would be unable to eat his dinner. How would he decide between albumen and earthy phosphates; between urates loaded with purpurine, and the oxalate of lime? Could he detect sugar in urine of low specific gravity without the test-tube? Could he diagnose a wasting, or a fatty kidney without attendant dropsy, if he dispensed with the microscope? or decide without this instrument that coagulation was due to the albumen of pus or blood if present in small quantity?

Now, what the microscope and the test-tube do for diseases of the balancing organs, the stethoscope and physical diagnosis do for diseases of the engines of life. We cannot understand how a physician in a right frame of mind can bring himself to disparage and treat derisively the means placed at his disposal by workers of the last thirty years for the investigation of chest disease; nor do we envy him the pangs his conscience must inflict for his systematic rejection of them in practice. We should like to meet the Doctor over a case of aortic regurgitation, or aneurism masked by chronic valvular imperfection. Would a diagnosis be arrived at by a due regard to the "vital force, power, or endowment," any more than to the colour of the hair? How will he decide between dilatation and a mitral regurgitation? How conclude between the former and a chronic pericardial effusion? between an old and a recent endocardial murmur? or, as simple a case as can present itself, dispense with fremitus, respiratory charac-



ter, and percussion, in diagnosing between pleurisy and consolidation?

And a strange contrast at once presents itself, in cruel mockery of the venerable views of Dr. Copland, if we consider the power in tracking disease, that we possess on the one hand, over the heart through its movements, the lung through its murmurs, and the kidney through its tell-tale stream; and, on the other hand, over those thoroughly respectable organs, the liver, spleen, and pancreas, which so discreetly keep silence when physicians libel them. Take, for example, the liver. Here is an organ resisting all attempt at physical investigation. As inaccessible as How-quah, it might, for all purposes of exact medicine and clinical progress, be like that celestial flower, in the moon. Like the King of Siam, it not only says nothing to anybody, but forbids anybody to say anything to it. Occasionally it protrudes a margin, and there's an end of it. It has, we are told, a secretion; of which, however, it declines to afford a sample. Accordingly, so little do we know of its pathology, that we scarce are informed of its function. In deciding, consequently, on its diseases, we are driven to symptoms, and symptoms solely. And what do they do for us? What do we purpose, when giving cholagogues? A pain in the shoulder?—Liver! Headache?—Liver! Black spots before the eyes?—Liver! "My dear Madam," says the Patriarch, "you are bilious—bilious, Madam, not a doubt of it." And though, of course, what such an authority says must be true, yet we would venture to ask each individual member of the profession, singly, and the entire medical body politic and corporate, collectively, what the word *bilious* means, and why, if it infers "flow of bile," we give mercury, which is, or was, supposed to aggravate such flow; and why, if it is not "flow of bile," we give precisely the same remedy.

Similar observations apply, when we reflect upon our knowledge of morbid changes in structure and function, of the pancreas and the spleen. Look through volumes of medical journals, go through ward after ward of hospitals, and you neither find narrated, nor see a single case diagnosed, "pancreatic or splenic disease." And why? Not, surely, that the pancreas or spleen enjoys immunity from disease; but that to the function, or the perversion of it, in either organ, we cannot bring the test of physical diagnosis. The brain is not so bad. It will often give tokens of the disturbances occurring within it. Yet oftener still does a perversion of its functions inflict nameless tortures, conjure up horrible terrors, or curse its owner with loathsome hypochondria, while the physician

can do nothing—powerless without the physical method. How many of us can detect and map out a lesion of the medulla above the anterior pyramids? Who can localize the *font et origo* of a facial palsy, of choreic irritation, of epilepsy? We experiment on everything with life. We turn ourselves into Manchos towards legions of rabbits and frogs, but do not arrive at a tithe of the knowledge of these organs that we have of the others; for comparative and experimental physiology will never tell us of them, what a drop of potash to the urine tells us of the kidney, or listening one moment with the stethoscope tells us of the lungs or heart.

The great truth at which those whom Mr. Meredith would call the WISE YOUTHS have arrived at is, to express it as a wise youth should, aphorismatically—*All disease involves anatomical change*. The question then presents itself, how to detect such change. Clearly by noting altered properties of substance and texture; alterations in elasticity, in density, in conducting power, in dimension, in physical and chemical characters. Such alterations always precede symptoms. A cardiac murmur is heard before dropsy displays itself. Sugar is present in the urine long before thirst and ravenous appetite arrest the attention. Anatomical change may or may not be susceptible of accurate discrimination; but its detection, at all events, is accomplished at a much earlier period, and with far greater accuracy, by observing altered physical qualities, than by the recognition of “vital functional derangement.”

The recognition of such changes is often very difficult, and thus far we are bound to sympathize with Dr. Copland. It is essential there be a skilled sense. But there must be more than this; for the signs in themselves are of no value, however accurately the sense may perceive them. The physician must be able to couple these signs with the condition originating them; and then, by means of a higher calculus, deduce *their* pathological bearings and character. To tread so far in the path of induction is no mean thing. For to know every step of the road is to solve and compass the problem, DISEASE; and were the *method* applicable to the remedies, man would be its master.

If then it be true, that in a clinical point of view the objective are of far higher value than the subjective phenomena of disease, it naturally follows that this superiority will be most marked in diseases of the chest; and this we believe is conceded by most physicians of the present day; for the lungs manifest physical signs during the whole of life, and under every condition of rest and motion, of health and disease, that

can possibly occur. Yet, in the book before us, the physical signs are, if not abjured, certainly disparaged; and the detection of the several stages of phthisis made to depend upon the symptoms! and this in the latest work of an authority—the work of Dr. Copland—of Dr. Copland, no longer the compiler, the accumulator of other men's wisdom, but the *teacher*! To account for such a fact as this, surely we shall find some startling symptoms of which the profession has hitherto been ignorant—symptoms which enable the author to dispense with signs in discriminating between capillary bronchitis and lobular pneumonia; between the latter without fremitus, and pleurisy; between the latter in the dry stage from pleurodynia, with bronchial irritation.

So that none of them may escape, we append them all. The treasures of experience are, in the book, respectively labelled A, B, C, &c., like the priceless porcelain goblins in the Dresden Gallery.

Treasure A.—*Quickness of respiration*. This is, as described by Dr. Copland, a sort of chameleon symptom:—"In the more latent and chronic states of the disease, it is not experienced unless more or less physical exertion be used;" and yet we have it, that "as the malady advances, respiration is very quick, the acceleration being much greater in proportion to that of the pulse than in health, or at an early stage."

Treasure B.—*Observations by the spirometer*, kindly furnished, not by the author, but by the Hospital for Consumption.

Treasure C.—*Hæmoptysis*, "a more or less dangerous occurrence," p. 29, "not often a very dangerous symptom," p. 40. It may occur "unexpectedly," or "suddenly," delicate alternative; and "immediately upon, or soon after, more or less physical exertion." We should have thought the above included every possible circumstance under which hæmoptysis could occur. But no! It may "on some other occasion" not be "preceded by any marked indication."

"When, however, circumstances admit of the occurrence, or of the appearance of premonitory symptoms, they" (the circumstances?) "are chiefly chills or *horripilations*, of the general surface, sometimes followed by redness and heat of the face, or *flushings of the cheeks*"—i.e. the face without the nose—"a feeling of internal warmth, particularly in the chest; pain or tension at the epigastrium or hypochondria; a burning sensation under the sternum, with more or less anxiety, inquietude, constriction, or oppression at the chest, or dyspnœa; a short dry cough, dyspnœa, or

shortness of breath on slight exertion; a dull pain or soreness under the sternum" (poor party, he must suffer terribly about the sternum), "between the shoulders, or beneath the clavicles; palpitations; a quick, hurried, or excited pulse, which is sometimes also hard, full, bounding, or oppressed; headache; coldness of the extremities, with a collapsed or empty state of the veins of the surface; lassitude, and sense of weight of the limbs; occasionally cramps, or spasms of the lower extremities; flatulence, or borborygmi; costiveness, or pale urine."

Of a surety, Dr. Copland, when he visited one mortal with all these torments, must have been getting up Dante's Inferno.

Treasure D.—*Cough*, which is to be considered phthisical according to "the state of breathing and the appearance of the features, *particularly of the eyes*." Further, we learn, that "gastric cough is readily mistaken for the *early stage* of phthisis"! and that "nervous cough is often occasioned by masturbation."

Treasure E.—*Expectoration*, whose characters pathognomonic of phthisis Dr. Copland states "are the striated mass, with a mixture of whitish fragments in it, and the ash-coloured globular masses which are observed in the more advanced stage." The quantity may be "almost altogether absent;" and "the cases of this description which occurred in my practice had been mistaken, *before I saw them*, for low nervous or typhoid fevers." With respect to calcareous concretions from the lungs, we are spared any of those dry details that could only serve to throw light on points of retrograde phthisis, peculiarly interesting at the present day, and are favoured instead with a touching reminiscence of a medical friend who spat up such things, and who was sent up the Mediterranean, in the first stage of phthisis, for change of climate, in the year 1818. He is now, we rejoice to say, alive; and we venture to congratulate Dr. Copland on his having, for so many years, enjoyed the friendship of such a gentleman.

Treasure F—is *Pain*, which, "when the bowels are disordered, may be caused by the state of the colon."

Treasure H.—*Pulse*, there being no treasure G.; it having, doubtless, been stolen for its value.

Treasure I.—*Hectic fever*.

Treasure K.—*The digestive functions*; in which all is said about such matters.

Treasure L—affords an opportunity for a brilliant solo on the gong. Emaciation justifies the inference of Phthisis, "when it occurs without manifest cause, under which circumstances

the *fuss*, *parade*, *manipulation*, and *charlatanry* of a physical examination of the bare chest, so often unnecessarily and even injuriously practised, may in most cases be dispensed with."

Treasure M.—*Clubbing of the fingers*. The doctor affirms this to be one of the most valuable gems of the collection. We think it paste. We know many persons with clubbed fingers who threaten to live as long as our friend, who "went up the Mediterranean in 1818."

Treasure N.—*Edema of the extremities*, which, when amounting to anasarca, renders it *not unlikely* (!) that there may be "albuminous changes in the urine."

Treasure O.—*Morning perspirations*. "When they appear, the second, if not the third, stage of the malady may be considered present. When this symptom is manifest, there can be no question of its nature."

Treasure P.—*Aphthæ*.

Treasure Q. This is the last treasure, and refers to the hair, with which naughty ones part very speedily, it seems; for "it often falls out very early, when the malady has been caused by masturbation, or premature or excessive sexual excitement."

And these are the symptoms which are to have priority of physical signs in the recognition of the several stages of Phthisis! We have never seen a smaller mouse produced from so big a mountain. The only symptom here mentioned, as discriminating the first and second stages, is morning perspirations; and is there any mortal man that ever felt a pulse, that would abide by such a symptom? Is it not universally conceded, that there is no symptom pathognomonic of a stage? We have seen, not one, but scores, of phthisical patients with complete dryness of skin. As for the "quickness of respiration," will anyone volunteer a corroboration of Dr. Copland's view, that the respiration becomes quick as the disease advances? We should, on the contrary, say that the disease manifests no such symptom; and it is certainly altogether at variance with fact, that the acceleration is greater than that of the pulse. Had Dr. Copland made a shot at pneumonia in speaking of the pulse-respiration ratio, he would have been nearer the mark.

We have scarcely patience to observe, that in our opinion nothing can be more trivial than the endeavour to give a clinical importance to hemoptysis as a means of detecting phthisis. Of course—for no one now believes in phthisis ab hemoptoe—when observed, it can only be sequential to rupture of tissue, to the attainment of such a phase of the disease as excludes the

hope of retrogression. And even as a symptom, the description of it is most imperfect and inaccurate. It is hematemesis that is attended with pain at the epigastrium, *not* hemoptysis; and something more than the state of the pulse, the paleness of the urine, and the inconvenience from borborygmi, are necessary to arrive at the conclusion, that blood is not being poured out by the stomach. We require more information than Dr. Copland vouchsafes, to decide on tubercles from the presence of mere bleeding, since it is possible for the blood to be florid from a large artery in the stomach, and for the blood to be frothless when voided in large quantity from the lungs. One of the most common corroborating symptoms, which is not mentioned, is the salt taste in the mouth. There is no mention of the influence of pregnancy upon phthisis; the habitual frequency of the pulse; the red line on the gums, insisted on by Dr. Thompson; periodicity in alteration of weight; and the relation of phthisis to glycoemia.

All these symptoms, however, are but the results of constitutional irritation; and it is easy to see that a large amount of deposit may be as productive of constitutional disturbance as a small amount of deposit in a softening state. Symptoms are merely the result of general disturbance secondarily induced; physical signs speak directly from the affected organ, and lead at once to diagnosis, to prognosis, and to treatment.

In nearly every possible case, the physical signs of the first stage may be held to be—in females, percussion dulness, with deficient expansion movement; in males, intensified vocal fremitus. Any one of these signs, if strongly marked on either side in either sex, is, excluding pleuritic adhesions in the case of movement, if not pathognomonic, more than sufficient to afford a clue to the interpretation of symptoms. If clearly established on the left side in the male, intensified fremitus is a most valuable physical sign. Marked dulness shows a deposit, which, of course, as the right side gives out naturally a deeper sound than the left, is most valuable if established on the left side, and is always present and to be detected, barring emphysema. In males, then, intensified fremitus, in females, deficient expansion, when allied with weakness of respiratory murmur as compared with healthy side, little clearness, interrupted and bronchial breathing, indicate the first stage of deposition and induration. What vocal vibration and deficient expansion with exaggerated elevation movement, do for the first stage, rhonchi and marked dulness do for the second. If the rhonchus partakes in the least of a cavernous quality, and the respiration is at all blowing, the diagnosis cannot be uncertain.

In the third stage, though a stratum of healthy or pneumonic lung may disguise the cavity where slight percussion is used, absolute dulness is to be elicited on strong percussion. If the vomica be empty, blowing or cavernous breathing may be heard; if not empty, the pectoriloquous character of the voice, gargouillement, and exaggeration of signs of first and second stages, exclude all chances of error.

We have not, in what we have here observed, attempted to affirm the infallibility of physical diagnosis. We question much whether medicine would materially benefit by the perfection of the physical method, *pure et simple*. There would be no room for a difference of opinion, so delightful to the old school. There would be no scope for those amiable *banalities* about power, vital endowment, and the constitution. Luckily for the peace of mind and self-gratulation of the oldest of the Achæians, physical diagnosis is in its infancy. There are but few even of its professed advocates who have educated the sense to the detection of the finer murmurs. And among the adepts, we are free to confess, it is possible for a diagnosis to remain uncertain, and for physical signs occasionally to mislead. Thus it is difficult, sometimes, to arrive at a diagnosis where bronchitis ushers in the tubercular disease. Phthisis is often masked by the co-existence of emphysema. We all know that the cracked-pot sound is not to be depended upon in all cases of cavity; that it may be present even when the lungs are healthy, and is not uncommon in pneumonia, with and without effusion. But the physical method will never allow us to ascribe, as we have seen done, the dyspnœa and weakness of amenorrhœa and spinal irritation to phthisis; or the fever and crepitus of the last stage to an attack of acute pneumonia. And in all doubtful cases, the practitioner skilled in the physical method enjoys an unmeasurable advantage over the practitioner judging merely by symptoms. For the former has these with which to check his results; the latter abides by the impressions these symptoms convey—symptoms which too often manifest themselves but at the last and irreparable stage of the disease, which frequently are common to many diseases, and which, in all cases, afford boundless scope for every possible fallacy.

It is not to be wondered at that Dr. Copland should give us no information on the histological character of the sputum. No man is wise at all hours, or on all subjects, or even on all branches of one subject; or he would have shown us wherein he differs, for he is sure to differ, from Dr. Andrew Clark and Professor van der Kolk. It cannot be of any con-

sequence to Dr. Copland what this latter gentleman thinks on the subject; but we have heard it said, that the Professor insists much on the diagnostic value of the expectoration; and that by a microscopic examination of it a "practical result" is arrived at, of a not unimportant character. He believes that even when physical signs are altogether absent, the elastic fibres of the lung are to be detected at the very commencement of the disease, and at the first formation of a vomica. It is, no doubt, very rash of van der Kolk to go so far as to affirm this sign to be one of the most valuable in incipient phthisis; and we have no doubt, that at Dr. Copland's suggestion, both he and Dr. Clark will recant so flagrant a heresy.

It was with eagerness we turned to the seventh chapter, wherein, as we observe in the table of contents, are to be found the "author's own observations on the state of the blood in phthisis." We entertained the rash idea that the chemical theory of tubercle would be sifted; that the non-absorption of fatty matters would be explained, and those subtle phenomena, of albuminoid selection be rendered easy things to understand. The vexed question of the inflammatory or non-inflammatory nature of the deposit; its distinctive feature as compared with other adventitious products; the reason why it has no inherent powers of growth; the presence or absence of nucleated cells in tuberculous matter; his own private opinion on the inter-corpuseular substance; whether there be a true blastema; why tubercles take so kindly to pigment as they grow older; and whether they can be said in this condition to approximate to cancer. Information on all these points will, no doubt, be detected by the earnest inquirer in the appended exposition of the author's own views:—

"I may adduce the following as the results of my own observations of the state of the blood in the early and in the advanced stages of phthisis:—At an early period, or even before the disease has fully declared itself, the blood is thinner or poorer than in health; the colourless globules are more or less abundant, and the red globules less numerous; the clot is somewhat smaller, its crasis less, and it sooner loses its cohesion. As the disease advances, and as febrile action is established, the fibrin is somewhat increased, and this is more certainly the case, if hemoptysis, or intercurrent inflammations of any of the pulmonary structures take place; the red globules are diminished, and the albumen and fatty matter are not very materially changed in quantity. The alkaline salts are slightly deficient, and lime is somewhat in excess. It should not, however, be overlooked, that, among the numerous analyses of the blood in phthisis, there are very great differences in the quantity of fibrin,



of albumen, and of fatty matter. I have here given what appears to be the more correct results. Probably the quantity of each of these is not so different as the quality, the intimate constitution, and vital relations. In the last stage of the disease, the blood appears still more watery, owing chiefly to the deficiency of red globules: and the colourless globules more numerous than in health. The greater abundance of colourless globules is probably owing to impaired assimilation, or metamorphosis, of these into red globules. The colourless globules in this, and indeed in earlier stages of the disease, have been mistaken for pus globules, the existence of which in the circulation is doubtful, or at least not satisfactorily demonstrated. The vital crisis, as well as the size of the clot, progressively diminishes, and the coagulating power or force of the fibrine is much diminished. It does not appear that the *microscopic appearances* of the blood in phthisis are different from those now stated, or that the observations which have been made with the aid of this instrument have furnished any additional facts to those now adduced."

"That the *vital crisis* of the blood is *specially altered* in tuberculosis and consequently in pulmonary consumption may be admitted; but in what the true nature of this alteration consists is beyond our present knowledge. We can only remark the appearances it displays, and the states of *vital force* and of assimilation and nutrition with which it is connected. That the tubercular deposits in the lungs or elsewhere, which certainly present the feeblest traces of organisation, even in an early stage of their formation, if indeed they possess any whatever (for both opinions have been espoused), should consist of a modification of fibrine, as supposed by Rokitansky, requires further confirmation. He ascribes the formation of tubercles to an arterial elaboration of the fibrine, and contends that, in consequence of the alteration of the fibrine, tubercle is continually deposited, even when the blood is deficient in that constituent, the fibrine that is formed being soon affected by the peculiar dyscrasia and deposited in the form of tubercle. The rapid coagulation of the tubercle-blastema, or the fluid from which the tubercle is formed,—for it must be effused in a fluid form,—its tendency, after coagulation and growth or accumulation, to soften, he believes to be favoured by an active arterialisation, and prevented by a venous condition of the blood—changes, which, in his opinion, indicate an affinity between tubercle and fibrine. This hypothesis derives its chief support from the circumstance that debility is found to increase the quantity of fibrine, and also to favour tubercular formations; but it carries us no further in our knowledge than was previously admitted—than that the increased quantity of fibrine in the blood of scrofulous and phthisical persons, arising either from the imperfect assimilation, or from the waste of the blood-globules, or from the waste of the tissues, being imperfectly endowed by the vital force, owing to its low grade, is insufficiently elaborated and eliminated, and is deposited in the form of tubercle in situations favourable to its accumulation, where its imperfect vital

endowment and elaboration favour the changes it subsequently manifests."

These are the author's *own* observations; and we beg they may be received with the deference due to "practical results." We should have imagined, had not Dr. Copland expressly told us otherwise, that these, for the most part, were Rokitsansky's observations, and not Copland's. They are, however, the author's *own*; and, as they are his own, we suppose, like the Duke of Brunswick's Own, they want no quarter. But what the Doctor declines we will in deference bestow. Otherwise we might confess we do not see much in them, and might venture a hint that tubercle is as much a cancerous exudation as a fibrinous, inasmuch as both these products have nuclei, and a tubercle corpuscle has not; that tubercle exhibits no resemblance to advanced fibrinous structure, either plastic or purulent, inasmuch as it presents no trace of organisation on the one hand, nor does the size of the tubercular corpuscle or its affection by acetic acid correspond in any way to the pus globule on the other. As a rule scarcely without exception, and then only when found among epithelial structures, tubercular matter presents no nuclei. This absence of nuclei, and presence of granular matter and molecules, holds equally for firm and soft tubercle; and the same elements are found to constitute the grey granulations, differing, notwithstanding, as these do so remarkably in appearance.

Nearly one-half of that portion of the work devoted to phthisis is given to treatment and remedies. It is edifying, if of doubtful practical value, to learn how phthisis was treated 100 B. C., and to find some countenance for our present differences of opinion in the want of unanimity in physicians of all times. Thus, those well-known practitioners, Oribasius, Actuarius, and Nomrus, advocate blood-letting; while Octavius, Horatianus, Cælius Aurelianus, Marcellus, Dioscorides, and Paulus, lend their authority to the administration of such elegant preparations as frankincense, pomegranate, and blood-stones. Celsus, we find, advises the cautery on various parts of the chest; and the ulcers *not to be healed as long as the cough continues*. Stolid old Roman! This is something like a "practical result." The elder Pliny was partial to goat's fat in gruel, evidently the cod-liver oil of the day. Galen invented a lozenge of *Scribonius Largus*, a title whose mystery gave it, no doubt, claims on public favour almost as strong as those of the *Revallenta Arabica*. (It appears, further on, that *Scribonius Largus* was an individual, not one of the *materia medica*.) Cælius,

like many foreigners of our acquaintance, considers the cold bath dangerous, and advises sailing to a distant climate. Otius was in soul evidently an alderman, for he had the highest opinion of venison fat dissolved in soup; and Schenck, who doubtless had attained higher civic dignities than that illustrious predecessor, is strong in praise of turtle broth, and snails fattened on sugar. Dr. Copland avails himself, in his poem on Bennet—not Hughes Bennett—of the opportunity of rebuking us once again. Bennet recommends Welsh flannel next the skin, “not to be too frequently changed;” and, says the Doctor, “If we except the recent employment of cod-liver oil in phthisis, in what, it may be asked, has the treatment of this disease been advanced, since the appearance of the work of Bennet, by the voluminous writings of specialists and stethoscopists of recent times?” Dover, we are told, advises a frequent bleeding, horse exercise, crude quicksilver in large quantities, aniseed and crocus martis made into pills, with the balsam of locatelli in the morning, and elixir of vitriol in the afternoon.

Dr. Copland's own treatment consists, if hæmoptysis is present, in sponging and the exhibition of turpentine. Cough he relieves by conium and camphor, and the *anisate of sulphur*. “In preventing, counteracting, or removing the morbid conditions of the circulating fluids in the advanced stages of phthisis,” the boracic and dilute phosphoric acids. The *alkalies*, “more particularly, when the blood is probably more or less contaminated by the passage into it of morbid matters from the lungs.” Ammoniacum and other gum resins, the fish oils, Carrageen moss, and issues, have his approval; and bleedings, under certain conditions, are not discountenanced. We append Dr. Copland's form for administration of turpentine:—

“R.—Olei Terebinthinæ ʒij. ad ʒss.; Camphoræ gr. vj. ad gr. xii.; Spirit. Ætheris Sulph. Comp. ʒss.; Tinct. Lavand. Comp. ʒiij. (vel Tinct. Guaiaci Comp. ʒiij. vel Tinct. Cinchonæ Comp. ʒss.); Olei Cajuputi mxx. ad mxx.; Tere cum Mucilag. Acaciæ ʒj.; Pulv. Tragacanth Comp. ʒij.; Pulv. Glycyrrh. ʒss.; Syrupi Rosæ, et Syrupi Tolutani, āā, ʒj. ad ʒij.; Aquæ Rosæ ʒij.; Aquæ Destillatæ (vel Infusi, vel Decocti Cinchonæ, vel Infusi Cascariellæ) ad ʒviiij. Misce. Fiat Mistura hujus sumatur Cochl. j. largum, omni horâ, vel ʒtis, 5tis, vel 6tis horis, prius agitatâ phialâ.

Dr. Copland insists strongly on the benefits to be derived from change of air, “travelling and voyaging.” As to the last of these, voyaging,—

“Very well founded expectations of success from it may be

formed, if it be commenced in this stage, or before the second be far advanced, more especially in cases which have been attended by hemoptysis at their commencement or early course, and if it be continued for a sufficiently long period."

A decent income, we presume, being taken for granted,—

"Voyaging in the Mediterranean or in the Atlantic between the degrees of latitude, 30° to 50°, and preferably in the Pacific Ocean, especially when prolonged, either in naval cruisers or by repeated voyages, so as to avoid the winter and spring of this and other countries unfavourable to consumptive patients, deserves to be more frequently recommended than it has hitherto been. Now that the passage across the isthmus of Panama is easy, voyages thence, in various directions in the Pacific Ocean, may be made, and a return to this country effected in May or June."

We conclude by quoting the Doctor on a most important article of dietetics, which would have made the elder Disraeli's mouth water:—

"*Milk* has always received great commendation in phthisis, but writers have differed respecting that which is most beneficial. Thus Zacutus Lusitanus and Blegny prefer *human milk*; Aretæus, Burserius and Stoll, asses' milk; Diemerbroeck and Velschius, goats' milk; Schenck, either asses' or goats' milk; Hippocrates, either *mare's* or asses' milk; and Heister, the whey of cows' milk. Stoll considered that *asses' and human milk should be diluted*, and that they are injurious in the inflammatory complications and in the last stage of the malady. Whatever may be the diet and regimen adopted, milk of various kinds, in suitable forms and states of dilution, constitutes an important part of the treatment of phthisis."

In bringing these remarks to a close, we hope Dr. Copland will acquit us of any desire to inflict pain, or to treat inconsiderately labours to which he has devoted so many years, and which he has pursued with such unremitting energy. As a man of vast learning, Dr. Copland enjoys the respect of every member of the profession; but no amount of learning can compensate such an utter want of judgment—a deficiency Dr. Copland never more needlessly or in worse taste displayed, than when he bethought him to characterize as charlatans those professional brethren who put a higher value than himself on the diagnosis of disease by means of physical signs.

*On the Origin of Species by means of Organic Affinity.* By H. FREKE, A. B., M. D., &c. Dublin: Fannin and Co. 1860. 8vo, pp. 135.

WHEN Dr. Freke's publication was placed in our hands for review, we were struck with the similarity that its title bore to Mr. Darwin's most interesting book. We had, however, not gone far, when we discovered that we were dealing with a very different writer, and that the similarity ceased the moment we passed the boundaries of the title-page. Indeed, no two books could be more dissimilar; and while Mr. Darwin's might easily find a place even on the elegant shelves of a lady's boudoir, Dr. Freke's was only adapted for the library of the philosopher and the student.

But why have called this little book "The Origin of Species," while it treats of no such thing? Mr. Darwin's work, indeed, pretends to account for how the present races of animals and plants have established themselves; but Dr. Freke does no such thing, and only attempts to show how life may have originated on this planet, when, during that period of solemn misty twilight, before the great luminary that rules the day had yet pierced through the heavy canopy of vapours that still enshrouded the earth—"The Spirit of God moved over the face of the waters."

We regret that Dr. Freke should have adopted so obscure a style of diction, as it only serves to render still more difficult a subject already difficult enough to understand.

Several years ago we reviewed a work of Dr. Freke's on "Organization;" and we were pleased to find that, in an age when men are apt to manufacture books by the aid of scissors and paste, he had started new and fresh ideas of his own, and, leaving the old and beaten track, had thought for himself. In the present work, which contains much of what might not inaptly be called the metaphysics of physiology, Dr. Freke has followed his old plan of thinking for himself; and though we cannot follow him everywhere, still we give him credit for his ingenuity, and the boldness he shows in expressing it, of which the chart placed at the commencement of the work is, perhaps, one of the most striking proofs. At first it was to us totally unintelligible, owing principally to some conventional terms introduced in it; but after having read the book, its meaning became clearer.

It is impossible, in the short space of a review, to give a satisfactory account of an almost metaphysical work, for the clear understanding of which, the paragraphs which *precede*

are almost always necessary for the understanding of those which follow, but we shall endeavour to give something of an outline of Dr. Freke's views:—

“Seek out” (he writes) “for one of the simplest and least complicated germs to be found in organic creation; ascertain the nature of its constitution, observe the nature of its function, and I feel assured we shall therein have discovered the nature, the constitution, and the function of every embryonic germ of the entire organic creation. . . . . Take the germ of some individual of the family of the fungi, and what do we find? We find a single grain, as it were, of dust, which when shed upon the *inorganic* earth, we see developing or generating a fungus identical with that which the germ has left.”

The function, therefore, of the *germs* is to confer organization, and as such Dr. Freke terms them in his book “organizing agents,” but their duty is twofold; the lowest germ, for instance, in the vegetable world, not only has to convert oxygen, hydrogen, nitrogen, and carbon, into an organic atom, similar to itself, but by its death, which is a necessary consequence of its having imparted organization to mineral matter, it gives rise to what Dr. Freke terms an “*organic residual product*,” which serves as *food*, or *specific stimulus*, to an atom higher in the scale of organization, in the same way as the inorganic compound of oxygen, hydrogen, nitrogen, and carbon had been the food or the specific stimulus of the germ of lowest organization; and so on, each successive elevation or development serves as pabulum or food to a higher organizing atom, until eventually we come to the highest of all, the organizing atoms of muscular fibre, nervous tissue, cerebral matter, &c., which substances are the *residual products* of their corresponding *organizing atoms*—these residual products by their death giving rise to the phenomena of animal life, mental phenomena, muscular phenomena, &c., and in doing so revert eventually to the four original elementary substances, oxygen, hydrogen, nitrogen, and carbon, which again, serving as specific stimulus to a germ of the lowest order, re-enter the great and endless chain of organic existence.

The lowest vegetable germ is, according to Dr. Freke, a *simple indivisible physiological atom*; as we rise in the scale of organization, the germs become more complicated, till at last in man the Graäffian vesicle is the most complex of all. It is in the diversity of the arrangement of the composing atoms of the several germs of the different plants and animals which inhabit this planet, that Dr. Freke recognises the diversity of

species, a diversity which must have existed from the commencement of time, when the great Architect of the universe clothed this earth with verdure, and commanded it to "bring forth the living creature after its kind, the cattle and the creeping thing."

In answer to the question of how did those complex germs arise, Dr. Freke tells us it was by a law of organic affinity, implanted in them *ab initio*, similar to the corresponding law in the mineral kingdom, and by which they congregated into various groups.

The difference between vegetable and animal life, according to him, is both simple and new. In vegetable life, each atom in reproducing a number of its own species *dies*, at the same time that it gives origin to a "residual organized product," which may go on accumulating almost *ad infinitum*, as for instance in trees, in some of which the successive layers of wood can be counted back for centuries of existence; for in vegetable life the only death required is that of the organizing atom; in animal life, not only does the organizing atom die, but also the organized residual product, which as muscle, or brain, or nerve, dies away, and reverts to its primeval inorganic components, in giving birth to its respective muscular, mental, or nervous phenomena.

We hope that Dr. Freke will continue to investigate this subject; and in the mean time we trust that physiologists will look into this little book, and not turn from it, if after the first few pages they should find its diction too obscure, or its matter difficult to understand.

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*Vestiges of the Natural History of Creation.* Eleventh Edition. London: Churchill. 1860. 8vo, pp. 286 and lxiv.

THIS is now the eleventh edition of a foundling work, the unclaimed offspring of a wonderfully clever imagination, beautifully written, full of interesting matter, most fascinatingly detailed, but, alas, not always true!

We regret that this eleventh edition should be so much inferior to the previous one, both in the paper and the print; the preface also has been omitted, which we look upon as a serious mistake.

An attempt has been made to bring the geological portion of it up to the present mark of scientific discovery, and at page 100 we read:—

"Since the above was written, English geologists have accepted a series of facts implying the existence of the human race at a stage of the Drift era, prior to the extinction of the animals of the bone-caves. At various depths, up to twenty feet, in the naturally-disposed chalk gravel, near the banks of the Somme, in Picardy, have been found multitudes of flint implements, rudely chiselled by the human hand, and apparently designed to serve as spear-heads. They obviously infer that a primitive savage people resided here before the deposition of this chalk gravel, an event of vast but unknown antiquity. To the same purport has been the discovery of similar flint implements in certain bone-caves, both in Sicily and England."

This work has been now for so many years before the public, and has been so often reviewed, that we shall not add another word either in its praise or censure, but shall close our review by a few quotations, which have some bearing on the subsequent labours of Dr. Freke—whose work we have been just reviewing—and who has endeavoured to elaborate the previous conceptions of the author of the *Vestiges*. At page 105 we find the following words:—

"The whole aim of science from the beginning has been to ascertain law; one set of phenomena after another has been brought under this conception, without one ever feeling that God was less the adorable Creator of his own world."

And also that—

"It is not easy to say what is presumed to be the mode of His operations. The ignorant believe the very hand of the Deity to be at work. Among the learned we hear of creative fiat, interferences, interpositions of creative energies, all very obscure phrases, apparently not susceptible of scientific explanation. . . . let the contrast between the two propositions be well marked. According to the first, all is done by the continuous energy of the Divine will—a power which has no regard to great or small. According to the second, there is a procedure strictly resembling that of a human being in the management of his affairs."

But God is the same yesterday, to-day, and to-morrow; and the same fires which upheaved continents in the early ages of our planet, are still working at present, upheaving the western shores of the South American continent; depressing the shores of Italy along the coast of Pozzuoli; and—

"in the ripple-mark on the sandy beaches of the present day, we see Nature's exact repetition of the operations by which she impressed similar features on the sandstones of the Carboniferous era. Even such marks as wind-slanted rain would produce in our day



on tide-deserted sands, have been read upon tablets of the ancient strata. It is the same Nature—that is to say, God through or in the manner of Nature—working every where and in all time, causing the wind to blow, and the rain to fall, and the tide to ebb and flow, inconceivable ages before the birth of our race, as now. So also we learn from the conifers of those old ages that there were winter and summer upon earth, before any of us lived to liken the one to all that is genial in our own nature, or to say that the other breathed no airs so unkind as man's ingratitude."

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*Quarantine and Quarantine Laws.—Return to an Address of the Honourable the House of Commons, dated May 22nd, 1860, for "Copy of Abstract of Regulations in force in Foreign Countries respecting Quarantine, communicated to the Board of Trade."* Blue Book.

*Return to an Order of the Honourable the House of Commons, dated 18th August, 1860, for "Abstracts of Returns of Information on the Laws of Quarantine which have been obtained by the Board of Trade."* Blue Book.

THE two papers, the titles of which stand at the head of this notice, contain a vast amount of interesting information regarding the quarantine laws, which has been obtained and prepared from official returns by the "Quarantine Committee of the National Association for the promotion of Social Science." In fulfilling this arduous task, the Quarantine Committee have performed a most valuable labour, having alike done good service to the interests of commerce and to the cause of sanitary science.

A more oppressive, vexatious, and at the same time absurd system of laws could not be contrived than that which has usually prevailed throughout the world, under the name of "quarantine laws." Based upon false principles, administered often in a partial, selfish, and ungenerous spirit, and too frequently casting aside the sacred dictates of humanity, how could these laws be other than cruel and oppressive to those who come under their operation? But, beyond all this, the restrictions which they placed, or still place, upon commerce, the false security which they give to the places where they are most stringently enforced, with a consequent neglect of all true sanitary operations, are calculated to produce, and have produced, most injurious effects upon the communities for whose protection they have been devised. The Returns before us fully, and in hundreds of instances, establish these statements.

There is scarcely a page from which we might not quote conclusive evidence regarding these several points; but a few examples will suffice, and we shall content ourselves by laying before our readers some extracts from "Papers relating to Quarantine Laws," and from "Papers relating to Quarantine Practice;" by which we believe the whole existing system will appear to be one of unmitigated evil,—full of fallacy, unsound, and untenable. We imagine, indeed, that in this country there is not much need to argue the question; nor shall we do so, beyond making some comments upon the evidence, as it turns up everywhere throughout the Returns now under review.

In Spain and Portugal, two countries notoriously negligent of the laws of sanitary science, the laws of quarantine are excessively stringent, unequal, and unjust.

Mr. Consul Mark, at Malaga, says:—

"Arrivals from Egypt with raw cotton are admitted to pratique after eight or ten days' voyage, while arrivals from England with a cargo of coals, and after a passage of twenty-five to forty or fifty days, are all quarantined for three days."

Again:—

"In 1853 and in 1854, the quarantine against Great Britain and other countries where the cholera existed, was most stringent in all the ports of Spain. In 1854, the pestilence, nevertheless, raged throughout the Peninsula."

Again:—

"Mr. Consul Mark, alluding to the evils, inconsistencies, and glaring absurdities of the system of Spain, as practised at Malaga, states:—'Under the specious pretext of preservation of the public health, our trade with Spain is subjected to a series of harassing restrictions and taxation, from which the commerce of other nations is exempt. It is mortifying to see vessels from healthy British ports forced to perform quarantine in the infected ports of this country, and to find arrivals from the British Isles suffering three days' observation all the year round.

"The evils of quarantine here are still further increased by the absence of all system, or unity of action, among the provincial Boards of Health; the law is interpreted according to the fancy of each Junta. The provincial Boards have repeatedly acted, each of them upon their own judgment, and in contradiction to the supreme Junta at Madrid. No injury to the health of the country seems to have followed, however vexatious to commerce.'"

In Portugal we find that "All vessels coming from the Brazils and the West Indies are more or less subject to qua-

rantine throughout the year. This quarantine is regulated by the Board"—the Board of Health at Lisbon, over which the Government has no control—"and is dependent upon the information received from the Portuguese Consuls at the ports of departure." "Arrivals from a port infected with yellow fever have to perform a quarantine of eight days; from a port suspected of the same, a quarantine of five days:" and this, too, although the ship's company are quite healthy! Nearly similar rules apply to cholera; and "These different periods of quarantine are irrespective of the length of the voyage, whether this has been only a week, or two months and upwards; and it would seem that they are very generally enforced." "At the beginning of this year, 1860, the whole coast of Brazil was declared infected with yellow fever, in consequence of its existence in one port; and a vessel which arrived from the port, Para, with sickness on board, was ordered 'to quit the port,' Lisbon, 'or *be sunk*, and the susceptible portion of the cargo now in the lazaretto to be destroyed!" Could there be anything more monstrous and absurd? Spain and Portugal, two of the dirtiest countries in Europe, admitting an arrival from the filthiest place in the world to free pratique, after an eight or ten days' voyage; while a quarantine of three days is enforced upon ships from England, though they had been at sea from twenty-five to fifty days!

As might be expected, the quarantine laws in force in the kingdom of the two Sicilies are or were "minutely elaborate and extremely rigorous,—more so, probably, than in any other country." In fact, in countries low in the scale of civilization, and under a tyrannous government, such a state of things naturally exists; but it is now to be hoped that a more enlightened system will prevail, and that, as in Sardinia in 1852, a great reform will take place in the quarantine laws. In fact, we believe that all thinking people will see the necessity for a marked modification of a system that has been proved, over and over again, to have been, not only perfectly useless, but even pernicious in its results. Strict quarantine has been enforced, and "sanitary" cordons, as they were miscalled, have been established when Cholera has prevailed, but "with the most signal want of success;" and we perfectly agree with the following views, thus stated by Consul Hunt:—

"After a residence of twelve years in extreme climates, the result of my experience is, that the epidemic diseases for which quarantine is usually imposed are exclusively the result of local atmospheric derangements, and that the spread of these diseases can in no case be prevented by the infliction of quarantine."

Moreover, by a reference to the Returns now before us we find, that in several states where the strictest quarantine had been enforced against cholera, but without success in excluding the disease, the rigour of the system has been greatly lessened, or quarantine has been abandoned; thus exhibiting a growing disbelief in the protection which quarantine had been deemed to afford. Then, again, we have in these Returns conclusive evidence that the necessity for quarantine has not existed, in almost any instance; for, in the lazarets, where passengers, crews, and goods have been landed, the disease against which the quarantine had been directed has scarcely ever appeared among the officials. For instance, take Lisbon, where the most severe quarantine has been enforced.—“The only cases of epidemic disease,” says Dr. Donnett, R. N., “that have appeared in the lazarets for the last forty-three years, are three cases of cholera, in July, 1856, whilst the disease was raging in Lisbon.” Similar evidence might be quoted from the records of almost every other lazaret, showing clearly that a system so pregnant with injurious restrictions on commerce should not have been enforced, and should no longer be tolerated. Plague, yellow fever, cholera, and small-pox, are the diseases against which the most stringent regulations have been adopted; yet the mass of evidence in these Returns shows the fallacy of the quarantine laws. In some instances these have been enforced against a disease supposed to be introduced, or coming from without, whilst it was raging within, fomented and cherished by the very system which was deemed to afford protection—for example, the last epidemic of yellow fever in Lisbon. In other cases we find nearly all the world restricted in its intercommunication in consequence of *reports* of disease existing in some out-of-the-way place, or from the prevalence of fever in a remote and filthy Moorish town!

It is much to be regretted that out of the learned International Quarantine Conference held at Paris in 1851-52, something more usefully determinate regarding the quarantine laws has not arisen; for, while the commission admitted the necessity for sanitary operations, properly so called, no abatement of the oppressive system of quarantine in force was proposed. Instead of regarding the subject in its true aspect, and recommending every country to attend to its own internal sanitary condition, as the only certain means of protection from disease, the Convention seems to have admitted this merely as a truth, but retained quarantine as a necessity. “It is satisfactory, notwithstanding the retention of such rigorous practices, to know that all the delegates of the Convention, with the exception only of

the Papal and Neapolitan, confirmed the important result of Sir W. Pym's observations in 1844, that not an authenticated instance of the plague has occurred in any lazaretto in the Mediterranean among all the men engaged in handling the cargoes of infected or suspected vessels, or of a case of the disease having ever been known to be introduced into a country by cotton bales, or other similar goods."

Now, like evidence is abundantly afforded by the Returns as regards yellow fever and cholera also. Hence, surely, the undefined bugbear of contagion should not be permitted longer to restrict commerce and cripple sanitary science. It is time that every country should awake from this dream of supposed security, and look to its internal condition, not imagining that epidemic disease comes from without, and can be kept out by any system of quarantine or cordons, but acting rather under the belief that the removal of filth, the causes of malaria,—in a word, the known impediments to health, are the only sure means of preventing disease from taking its rise in, or spreading through a community. The rapid intercommunication, indeed, which now exists through the agency of steam, must soon lead to the breaking down of the established quarantine laws, and the enactment of others more consonant with justice and common sense, and more compatible with the interests of trade. In aiding this most desirable consummation, these Returns must hold a prominent place, as they exhibit to the most prejudiced and timid, data which should be conclusive regarding the fallacy, the inequality, injustice, and, in some respects, the inhumanity of the recognized method of quarantine. But, beyond this, these Returns also in the most decided manner make reference to the marked influence which neglect of sanitary operations exercised everywhere in the spread of epidemic disease, and also satisfactorily show the salutary effects produced by them whenever properly carried out. In fact, the Reports furnished by authority from almost every part of the world, give clear evidence upon these points; and, although several of them seem still to favour the imposition of strict quarantine, they all agree that both on board ship and on shore much greater attention than has hitherto been observed should be paid to the causes which affect health, and either induce or propagate disease.

For our own part, we believe that were a stricter attention given to sanitary science—were its salutary principles diffused, generally received, and acted on, the spread of disease would be very much limited, and its ravages greatly lessened; and hence we also believe that the quarantine laws could be with

perfect safety either totally abolished, or so much modified that they would not, as at present, injuriously affect commerce, and be a vexatious nuisance in many parts of the world. Whether all civilized countries will soon be led to adopt these views, it is impossible to determine. We know that it is difficult to overcome old prejudices and ancient customs—still more difficult to do away with systems in the maintenance of which many officials have a direct personal interest; yet we are perfectly satisfied that the spread of sanitary knowledge, the increase of national intercommunication, and the observation of the inefficiency of quarantine regulations will, at no very distant date, lead to a generally uniform, liberal, and simple system of quarantine, if any such system should still be considered judicious or desirable. And, as we have already said, the Returns now under notice will tend greatly to open up the question, and to diffuse sound knowledge regarding a subject hitherto but little understood. No one can rise from carefully perusing the evidence they afford, without being deeply impressed by the important information they contain, and satisfied also of the candid spirit in which the Quarantine Committee of the National Association for the promotion of Social Science have made abstracts of these returns. The labour of examining the vast amount of documentary evidence, and of making these condensed but clear Reports, must have been very great; but we feel certain the gentlemen who undertook this arduous task will have the gratification of knowing that their work has not been in vain. We regard it as one of those philanthropic undertakings in which this country has so often been the pioneer, and which are of such social importance to the human family. The spread of sanitary science, the inculcation of sound sanitary principles, is a work of vast importance; and every man, every association of men, who aids its progress must be regarded as filling the place of a public benefactor; for, what undertaking can be more beneficent than an inquiry into those laws which affect the social condition of man, and with which his health and happiness are so closely allied?

*Manuel de Médecine Operatorie.* Par J. F. MALGAIGNE, Professeur de Médecine Operatorie à la Faculté de Médecine de Paris. 1860. 12mo.

M. MALGAIGNE has, in the work before us, endeavoured to do for the French students what Mr. Fergusson had already done for ours in his "System of Practical Surgery." The book is the same size, the same thickness, and has even the same number of pages; but it lacks the beautiful illustrations which add so much to the value of our English publication, at the same time that it is deficient of much valuable information. The print is also considerably larger; and therefore what is, perhaps, gained in clearness of text, is lost in subject-matter. The first fifty-six pages are devoted to the consideration of the more elementary portions of operative surgery; the description of instruments, and their uses; the application of caustics, and the several modes of suppressing hemorrhage; terminating with a short chapter on etherization, in which he warmly advocates the use of chloroform, asserting that in his hands it has never failed to produce the desired effect; and that he never had the misfortune of meeting with an accident. He applies it simply on a folded cloth, taking care to allow the patient plenty of air to breathe at the same time. Speaking of the *écraseur*, he says "Although I do not admit all M. Chassaignac's assertions concerning it, yet it appears certain that, if carefully applied, it will effect the division of tissues without hemorrhage, at least when they are traversed by arteries of moderate caliber. I doubt, however, its becoming universally employed -- first, in consequence of its complicated nature; and, also, from the danger of its breaking during an operation, as I have seen happen." The fourth chapter of this introductory portion is devoted to the consideration of cauteries, potential and actual. The hot iron, which once used to be the surgeon's sceptre, has fallen too much into disuse in these countries; not so, however, on the Continent, where it is still much employed. There can be no doubt that, under judicious management, it is a most useful agent for good.

While speaking of abscesses, Malgaigne recommends that such as happen about the face should be allowed to break of themselves, "to prevent the formation of cicatrices." But it appears to us, that to allow an abscess to ulcerate its way to the surface would be about the best way of ensuring a well-puckered cicatrix, which might be avoided by an early incision while the skin was still sound.

The article on transfusion of blood is interesting, though

too concise, which is, indeed, the main error of this Manual. M. Malgaigne remarks, that although J. Hunter, Scudamore, and J. Davy, have fully demonstrated that blood coagulates sooner in heat than in cold, still directions continue to be given in systematic works on surgery to keep the blood warm during the process of transfusion, in order to prevent its coagulation.

The article on aneurisms is also much too brief. In it the author advocates compression in preference to deligation, although, from statistics gathered by himself, he found 70 cures out of 108 popliteal aneurisms treated by ligature, and only 61 cures out of 108 similar cases treated by compression. He is of opinion, that the best treatment will consist in diminishing the current of blood into the aneurism for the first few hours, and then, by increased pressure, stopping it altogether. He also recommends to apply a certain amount of pressure on the distal side of the aneurism, to prevent, if possible, the reflux of blood into the sac from the collateral circulation, to which reflux the majority of failures is due.

In describing the operations for tying the arteries of the lower limbs, he is so extremely laconic, that he is very nearly useless to the student; and in his account of the several modes of taking up the femoral artery in its upper-third, he completely ignores Dr. Porter's plan of a tranverse incision, by which the vessel can be tied much higher than by the longitudinal section, at the same time that it greatly facilitates the finding of the artery. Malgaigne's account of the operation for the removal of the upper jaw is also most unsatisfactory. After detailing, at greater length than usual, Gensoul's mode of proceeding, he merely mentions that Fergusson performs this operation with a single incision through the soft parts, without adding a word of the many improvements introduced since the days of Gensoul, to whom, certainly, is due the merit of having first executed this formidable operation; and the student who learns his operative surgery from Malgaigne's Manual would proceed to this operation encumbered with chain-saws, chisels, and mallet; and would disfigure his patient (should he even succeed in the operation) by leaving on his face the traces of three incisions, instead of one. As for the mode of securing the patient, or as to the propriety of supplying him with wine during the operation itself, or whether chloroform is admissible under such circumstances, he does not say a single word to guide the young practitioner. While on the subject of amputations, the author gives us some interesting statistics, which must be very gratifying to us; for in-



stance, he says that in Paris there are 50 per cent. of deaths after non-traumatic or pathologic amputations, while in London the mortality is only 22 per cent. Amputations of the thigh show a mortality in Paris of 60 per cent., and in London only 24 per cent.

Speaking of amputation through the wrist-joint or radio-carpal dis-articulation (an operation which is rarely, if ever, performed in these countries), he says, that he much prefers it to amputation of the fore-arm; first, because it leaves a longer stump, which is a fact of much importance to the working man; second, because, according to him, the mortality is much less than in amputation of the fore-arm. Out of nineteen cases collected in the hospitals of Paris, he counted eight deaths; whereas, in sixteen cases of radio-carpal dis-articulation, not one death could be enumerated. If these statistics could be relied on, they would be quite sufficient to cause the adoption of the latter operation in preference to amputation above the wrist; but we think there are still further reasons to recommend the adoption of this operation in appropriate cases, viz., the greater length of the stump, together with the retained power of pronation and supination. Liston, one of the greatest surgeons of modern times, says—"Amputation of the fore-arm should not be attempted *below its middle*, otherwise a good covering cannot be had for the ends of the bones." Fergusson, of King's College, says of this operation, radio-carpal dis-articulation, "Surgeons in this country have had but little experience of it. . . . I should certainly wish to see the practice more frequently tried than has yet been the case." He adds, further down, "I should certainly cut off the articular surface of the radius, if a saw or forceps were by me at the time." Malgaigne, however, does not say a word about removing the articular surfaces in this operation more than in amputation through the elbow; and yet, he gives the most encouraging statistics for both, and concludes his article on the latter operation by saying, "*la supériorité de cette opération sur l'amputation du bras est donc désormais hors de question.*" Here, again, Malgaigne is at variance with Professor Fergusson, who states, "Notwithstanding the sanction of so high an authority as Dupuytren, who performed this operation frequently, it appears that there are few advocates for it in the present day; and, for my own part, if I did perform it, I should most certainly cut off the articular surface of the humerus." If Malgaigne's statistics are to be believed, and we can see no reasons for impugning their accuracy, the plan of amputating through the wrist and elbow-joint should receive more attention in

these countries than has been given to it; and certainly a fair trial to the operation, without resection of the articular end, should be given it, because every plan which tends to simplify a proceeding, by diminishing the number of instruments required, is undoubtedly a valuable improvement.

The next operation we come to is one which we trust never to see performed in this country—paracentesis of the pericardium. We must, however, do the author justice, by saying he does not in any way recommend it, at the same time that he does not condemn it. It appears to have been performed in France by no less than seven different surgeons, including Desault, Larrey, Jobert, Trousseau, and Aran. Different intercostal spaces were selected by each; and Aran even injected a solution of iodine into the pericardium of his unfortunate patient, adding, “et le malade ne parait pas s'en être mal trouvé;” though, from the absolute silence as to the result, we take it for granted, that although the patient did not feel very ill after the operation, yet the case ended in death.

On the whole, this Manual is so very much inferior in every way to all our standard works on surgery, that, although it may prove a boon to the French student, it would never repay an English one the trouble of reading it.

*Clinical Memoirs on Abdominal Tumours and Intumescence.*

By the late DR. BRIGHT. Reprinted from “Guy's Hospital Reports.” Edited by G. HILARO BARLOW, M. D., M. A., Cantab. The New Sydenham Society, 1860. 8vo. pp. 326.

THE New Sydenham Society have done well in presenting to their readers this collection of essays by the late lamented and distinguished writer, Richard Bright. Few men in their generation have done more for their fellow-man than the late Dr. Bright; painstaking, accurate, conscientious, endowed with rare powers of observation, and gifted with a facile pen and high descriptive powers, all that he has ever written is endowed by these qualities with a high value in the estimation of his professional brethren; and we are of opinion that his editor, Dr. Barlow, has acted with a sound discretion in confining himself to the simple task of reprinting the essays as they appeared from the author's pen in the Guy's Hospital Reports. That he is not ignorant of the progress that medical science has made in some of the subjects treated of in these essays, is evidenced by the following passage that we extract from

the preface; while, at the same time, the argument put forward for not tampering with them, meets with our cordial concurrence:—

“The papers in question being essentially clinical, and consisting chiefly of well-grouped examples of the diseases under consideration, it has been considered better to reprint them almost exactly as they were originally published. The memoirs themselves had no pretension to constitute a complete monograph on the subjects which they illustrate. To have attempted to make them such by the addition of notes or interpolated material from other sources, would have been to wholly change the character of the work. It would have taken it beyond the scope of what was contemplated by the Council of the Society, and would also in some degree have deprived it of its greatest merit—that, namely, of being clinical portraits, fresh from the hand of a master. The task of editing has, therefore, as far as the body of the work is concerned, been restricted to the careful correction of verbal errors and obscurities of expression, and the re-arrangement of the plates, so as to bring them into juxtaposition with the cases to which they belong. In the original papers most of the illustrations were on stone, and were appended at the end of the volume; in the present reprint they have been reproduced in wood (by Mr. Tuffen West), and are incorporated with the text, so as to facilitate reference. The addition of the List of Cases, which is given at page xii., and of the Index, at page 323, will doubtless be found useful.

“In a few subjects, such, for example, as the histology of the acephalocyst hydatid, great advance in knowledge has been made since the publication of Dr. Bright’s papers. It has been thought best, however, even in these matters, to adhere to the rule stated above, of not making any additions to the original statements.”

Many of our readers, doubtless, will remember the substance of these essays, as they appeared in the Guy’s Hospital Reports; to them their re-appearance in a collected form will be now acceptable; to such, however, who may not either remember them, or who have not before seen them, we may be permitted briefly to indicate the nature of their principal contents.

Dr. Bright’s object was to bring his vast clinical experience and great diagnostic tact to bear on the elucidation of confessedly a most obscure department of medical disease—the discrimination and diagnosis of abdominal tumours; and this he has done by briefly stating the principal characteristics of these, as they are produced, either by the presence of tumours dependent on acephalocyst hydatids, by ovarian tumours, or diseases of the spleen, liver, or kidney; under each of these heads, we have valuable features recorded, by which in life they may be recognised; whilst after death their pathological

characters are described in a manner that leaves but one impression on our minds, that here indeed the author has held up the mirror to nature; and under each section we have a perfect *embarras de richesse*, in the shape of illustrative cases. The whole work is profusely filled with woodcuts and outlines descriptive of the several diseases described, by which means the author's verbal descriptions are more vividly presented to the reader's understanding. In the first chapter, which is devoted to "the exploration of the abdomen," we find a faithful description of the contents of this cavity, as they should be *in situ* in the healthy individual; and it concludes with a passage of such practical value, that we are tempted to reproduce it here for the benefit of our readers:—

"With a view of assisting in registering facts, it appears very desirable that every one who is really anxious to make the most of the experience which comes within his reach—a duty which, unfortunately, from the time it occupies, we are all too apt to neglect—should provide himself with some ready mode of transferring to the corner or the blank page of his note-book an outline of the abdomen; upon which he may mark, as nearly as possible, the exact position of any tumour which he is called upon to treat: and, for this purpose, I have employed one or two different little contrivances, which it may not be amiss to mention. In the first place, having drawn on a thick sheet of paper the outline desired, we may, with a pin, make holes in a few prominent points; and pricking the note-book through these holes, the least-experienced draftsman will be enabled to make an intelligible sketch in a very short time. I have likewise had the figure cut in a brass plate, to use it in the mode of stencilling; and have thus procured, in a few seconds, upon any part of the page, such an outline as is represented in Figs. 3 and 4. Again, it would be a matter of a very few shillings' expense, to have a woodcut or type formed, which might be used like a seal, even with common ink. It is obvious that no one single sketch can serve for every case; because the relative proportions of the different parts of the abdomen are somewhat altered, as it becomes distended, and consequently thrown out of its natural form; but still, the convenience of some such mechanical contrivance is very great, and there is no difficulty in being provided with more than one form of outline; and perhaps a second, representing the moderately distended abdomen, would be quite sufficient for every purpose. It will be at once perceived, by a reference to the figures, how the situation and extent of tumours, whether visible to the eye or ascertained by the touch, may be traced on the outline; and thus remain a fixed record, by which to judge of the progress of the individual case, or afford a means of comparison with others."

In conclusion, we have to congratulate the New Sydenham Society on the creditable manner in which these memoirs have

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been produced, and their editor on the able manner in which he has discharged his task. In these days of publishing mania, it is pleasant to find parties that think of *republishing* standard works, such as these; and we only trust to see this spirit more widely diffused. At the moment the thought forces itself on our minds how gratefully we should feel to any one who would bethink himself of giving us another issue of that mine of wealth—our own old Dublin Hospital Reports—five volumes which, in our opinion, might challenge competition with any similar publication in any language, but which are now out of print, and consequently, almost out of the reach of our rising generation.

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*Over de Allantois en hare Vorming en Veranderingen in den Mensch.* Door J. L. C. SCHROEDER VAN DER KOLK. Uitgegeven door de Koninklijke Akademie van Wetenschappen. Met eene Plaat. Amsterdam. C. G. van der Post. 1860. 4to, pp. 36.

*On the Allantois and its Development and Changes in the Human Subject.* By J. L. C. SCHROEDER VAN DER KOLK. Published by the Royal Academy of Sciences. With a Plate.

THE contributions to science of the distinguished author of the above essay are too numerous, important, and well known, to render it necessary for us to say a word in commendation of the manner in which the writer has on the present occasion performed his task. Suffice it to state that the same care and fidelity, and beauty of illustration in his admirable drawings, so conspicuous in the larger works which have from time to time emanated from his pen, are manifested in the paper on the Allantois. For details we must refer to the work itself, remarking only that among the most important points contained in it is the statement of the discovery that in the commencement the Allantois has a sort of villi, and that subsequently its sac separates from the stem and collapses, and that we thus have a guide to a good explanation of the abnormality known as the inverted bladder.

*A Treatise on Fever; or, Selections from a Course of Lectures on Fever.* Being part of a Course on the Theory and Practice of Medicine delivered by ROBERT D. LYONS, K. C. C., M. B. T. C. D., M. R. I. A., &c., &c.; Physician to Jervis-street Hospital; late Pathologist-in-Chief to the British Army in the Crimea, &c., &c. London: Longman. 1861. 8vo, pp. 473.

THE subject of fever must always command and obtain attention. It is emphatically in the art of medicine *the* subject, important in all its aspects, whether local or general, individual or social. It is the foe which carries on from the first an internecine and truceless war against the human race, pulling down the strength of the strong man to the helplessness of the child; and, in the touching language of England's great philosopher, "stripping the mind of its ideas, and in a few days calcining by its flames all those images to dust and confusion which seemed to be as lasting as if graved in marble." Too often we see it, at home with us, following in the footsteps of famine, sweeping away the wretched inmates of the peasant's hut whose debilitated frames offer but little resistance, or startling us by its appearance in the higher walks of life, reaping the summer corn before it is ripe. We see it again as the scourge of camps, more deadly than all those scientific agents together which ambition has invented for legitimate slaughter. As the "Kriegs-pest," or war-plague, we have read in our boyhood of its stealthy and unsuspected ravages in the Grecian camp, in that glowing Epic which describes the *earliest* siege on record; and in the *latest* which has occurred, we have painful evidence in the work before us, that medical science has not yet succeeded in disarming her terrible enemy.

Since the time of Hippocrates, much has been written on this disease; but, as regards its hidden nature, it still merits the name of "the pestilence which walketh in darkness." Yet this ignorance of ours does not proceed from any lack of the spirit of inquiry; our book-shelves labour under the great variety of speculations about its essence and operations. Indeed, as Judge Jeffreys once said of the writings of Richard Baxter, the bare theories of fever "would fill a cart." And it forms a curious illustration of the vanity of human expectations, that in the numerous theoretical works on this subject, it is precisely the portion most elaborately wrought which soonest falls into disesteem. The valuable practical facts have been carefully stored up from age to age, while the theories have been winnowed away. Some pathologist proposes to the world an in-

genious hypothesis, which, he thinks, appropriately colligates the facts, and this holds good until experience shows that the bond is fragile. Still we should not discourage such researches because they are conjectural. It is said that the famous Kepler made nineteen guesses before he discovered the true planetary curve.

The work on fever now before us consists of *excerpta* from a course of Lectures on the Theory and Practice of Medicine addressed by the author to his class. He gives therein the results of a large experience in the observation of fever, beginning with Ireland. Unhappily, this country possesses the unenviable distinction of being the land of fever; as the author graphically describes it:—"Typhus, chief habitat, Ireland." Can it be that the Gælic branch of the Celtic race possesses a constitutional susceptibility to this disease more than the Cymric variety, and much more than the Anglo-Saxon? Or are we to look to a plurality of causes, indigenous to Ireland, but less prevalent elsewhere, for an explanation of the phenomenon? In the terrible famine fever of 1847 and subsequent years, the author, in common with his brethren over the length and breadth of the land, had the sad privilege of studying it in its most characteristic form. He had also, by his appointment as Pathologist-in-chief to the British Army in the Crimea, possessed many facilities for studying the "*bellica febris*" or war-typhus, both in the camp before Sebastopol, and in the hospitals at Scutari. Lastly, the author visited Lisbon, in 1857, to investigate the epidemic of yellow fever prevalent there, and has furnished us with important records, taken on the spot, of the pathological anatomy of this group of fevers. He is thus enabled from personal observation to sketch vividly the lineaments of these different, yet similar forms of disease.

"Facies non omnibus una,  
Nec diversa tamen, qualem decet esse sororum."

From such opportunities, duly appreciated, we should expect both valuable and original matter; and after a careful perusal of his book, we are not disappointed.

The present work comprises eleven chapters, of which the first two are devoted to general observations on fever and its pathology; the third, to the classification of its specific forms, which are reduced under three great types, viz., Primary, Irritative, and Eruptive; the five next are taken up with a description of simple continued fever, the varieties of synochal fever typhus, typhoid, and the fever of the Crimea, which he regards as the typhoid form. Having concluded the subject of con-

tinued fevers, he should next, according to his classification, have taken up *seriatim* the intermittent and remittent subdivisions of primary fevers; but he passes over the first in a few pages, as not coming within the scope of his work, and discusses in the closing chapters Yellow Fever as a branch of the paludal form. It will thus be seen that Dr. Lyons does not profess to treat of fever in its widest signification; for example, of the above-mentioned three leading groups, he does not attempt the two last at all viz., the irritative and eruptive; and but partially the two orders of the class primary. We have here ventured to reverse the nomenclature of the author, who, in the heading of the ninth chapter, has seemingly by a technical oversight written thus—"Order 1. Primary Fevers; Class III.—Remittent Fevers,"—as his *subsumption* (as logicians call it) of *class*, as the less general expression, under *order* as the more general, is opposed to the use of these terms by the best writers on natural history.

With respect to the pathology of fever, it is almost universally admitted that the great central phenomena of this disease to which the others converge, is the production of animal heat above the normal standard. It is curious how closely general opinion here coincides with scientific research. We trace the one striking condition in the ancient epithets which have been applied to this disease; thus, the *causus* of Aretæus, the *πυρετός* of the Greeks, the *febris*\* of the Latins, were names all borrowed from sensible images expressing heat. This morbid calorification is, of all the symptoms of fever, the only one which is never absent in some or other of its phases. Dr. Lyons has given his adhesion to the views of Virchow, of Berlin, which are based on this foundation—

"Though," he says, "I am far from saying that they are to be considered as giving us a whole and complete solution of the pathological problem, what is fever? they may, I think be safely accepted as embodying a large amount of what is most essential for the ultimate determination of this question."

In what may be called the *common-sense* view of fever there are three elements associated together, namely, increase of temperature, frequency of circulation, and general wasting of the body. These form the fundamental positions of Virchow, but he endeavours to make the chain complete by additional links. We may state his views from the very clear outline of them

\* We cannot agree with Dr. Copland, who derives *febris* from "*ferbo*, or *februo*, I cleanse;" it is, as Varro observes, from *ferveo*, to be hot,—*fervor*, heat.



given by Dr. Lyons, briefly as follows.—He assumes, not without reason, that the morbid production of animal heat is the *essence* of fevers. The fact, at least, that there is such an increase of temperature, is undeniable as not only the unaided senses show it, but the thermometer also indicates a rise of several degrees above the natural standard in the interior of the body. Where, then, are we to seek for the cause of this augmented temperature? Is it in the nervous system, the mass of the blood, the respiratory function, or the nutrition of tissues, that the *point de depart* begins? In the present state of medical science we cannot speak with certainty. But, whether the elevation of temperature arises from a single cause or a plurality of causes, the mass of the blood, pervading as it does all parts of the system, comes very soon to share in it. Looking to analogy, we should be led to seek for the elevated temperature of the febrile state “in an increased activity of the causes which operate in the production of the physiological or healthy temperature;” and as we know that the normal production of heat is satisfactorily explained by changes in the nutrition of tissues, it is fair to conclude, by a parity of reasoning, that abnormal heat should result from abnormal changes in the tissues. This is an important step in advance. Virchow connects increased heat with increased consumption of organic matter; the mysterious chemical apparatus going on within our bodies is disordered; it wants fuel, and, failing to obtain materials from without, it uses up the elements within, both fluids and solids undergoing metamorphic changes to a remarkable extent, as evidenced in the wasting which goes on *pari passu* in the outer parts of the body. We have now to connect the above phenomena with disordered innervation.

“Taking into account,” says Dr. Lyons, “the various phenomena which attend the onset of fever, characterised as it is by weakness, prostration, and loss of energy, especially in the muscles and sensitive apparatus . . . we may conclude with Virchow that the elevation of temperature which is found to be amongst the earliest as well as most constant of the symptoms, is a *paralytic* phenomena. Though not identical with the ordinary paralytic state it is in all essential respects analogous to it, and can only be produced by a loss of power in the nerves which constitute the natural regulators, or it may be moderators, of the development of animal heat.”

There are some important arguments founded on experiments in support of the preceding views, rendering it probable that, as Cullen long ago maintained, the first step in the establishment of febrile action is to be sought for in the nervous system. Upon such a supposition, the order of the phenomena

would be this, viz.:—nervous disorder, increase of circulation, augmented nutrient metamorphosis of tissue, elevation of animal heat. We would thus have a chain of sequences, of which the last can be affirmed of the first. How then, it may be asked, are we to suppose that the nerves can regulate the animal temperature? By the intervention of the vascular system over which the nervous system exercises, by the vasomotor nerves and in other ways, a great influence. Here experiment steps in to our aid. It renders probable the supposition that in the animal body the development of heat in the normal state is kept in bounds by certain nerves; and that in fever, when this curbing power is enfeebled, the heat rises. Thus Bernard's experiment of dividing the sympathetic nerve in the neck was followed by a rise of temperature in the corresponding half of the head. Dr. Lyons has brought an interesting body of evidence to bear on the same point; and he renders it highly probable "that the vagi nerves, and we would add the sympathetic, are primarily, if not chiefly engaged in the production of the febrile phenomena."

But we have one step more to advance: we trace back the vagus nerve to its origin, and we there find the centre of this regulating property, which in Virchow's opinion resides in the medulla oblongata, near the roots of the vagi.

To sum up the preceding remarks—the weight of the fever poison would seem to be primarily exerted upon those centres of the nervous system which regulate the consumption of the nutrient material, and to produce there a degree of irritation which tends to arrest the controlling power.

"As the power of the moderating centres is arrested by the abnormal tension or irritation, the consumption of material advances throughout the tissues, and in proportion the heat of the body increases, and the particular point of the commencement of fever is reached. At first we see only the weakening of the corporeal and mental powers, which follows directly on the condition of tension or irritation being induced; and this in some instances with such suddenness of the prostration of the vital powers as to suggest the idea of a true paralytic invasion of the nervous system."

We have already observed that Dr. Lyons reduces fever under the three great types of primary, irritative and eruptive. Acknowledging the imperfect state of our present knowledge of the etiology and general pathology of fever as an impediment to a classification upon a scientific basis, he states that "he considers the proposed arrangement as being free from grave practical objections, and at once simple, comprehensive,

and readily committed to memory." We fully coincide with the author upon the difficulty in question, and are of opinion that it is easier to criticise any given classification than to propose a better; still, as a good classification of any subject is a great aid both to accuracy of knowledge and to the memory, we are not in favour of the simplest arrangement, if it be not also the best. Taking pyrexia as a general characteristic of the three groups, the author makes the essential difference of primary fevers to consist in an absence of specific pathological lesion, "and as having no necessary connexion with localised disease in any portion of the body, or in any organ or tissue." The irritative fevers, he observes, have an anatomical seat, "and owe their existence to lesions of a well-defined kind in particular parts of the system." Again, the eruptive fevers are without lesion of the viscera, while the cutaneous surface is the seat of pathological changes. Now, it may be objected to this division, that granting the basis to be correct, (which in primary fevers is not so generally admitted) the laws of division are not strictly observed, as the three groups are not sufficiently *opposed*. A legitimate division of fever upon the basis of the presence or absence of local lesion should be made primarily, it appears to us, into two groups; viz.: those *without* any local lesion, primary fevers; and those with manifest lesion or change, viz.: irritative and eruptive fevers—the lesion in the two last differing in the *seat*, which is internal or visceral in the first, and external or cutaneous in the other. Whether the author has taken a proper ground of distinction in this test, is another matter altogether. Again, on his basis of classification, exanthematic typhus should be classed under eruptive fever; and though he makes the relation of their phases to *time* to be a sufficient ground for separating primary into continued, intermittent, and remittent, he is obliged to resign this element of distinction, and place one form of remittent under another group. But, while we make these remarks on the author's classification, we would be sorry not to do him the justice of observing that the subject admits of so many cross-divisions, it is difficult to group the forms under suitable heads without violating rule. The varieties of fever are so numerous, especially the climatic forms, that a table of nosology, in order to include them all, should be so large as to be cumbrous.

We need not dwell upon that form of inflammatory fever which has long been a puzzle to students under the name of synocha. It has had the ill fortune to be regarded as a myth, and to be rejected without scruple from the catalogue of fevers; and French writers have had the hardihood, according to Dr.

Lyons, to pronounce that all the synochal fevers from Hippocrates down are gastro-enteritic fevers in disguise. The author, however, stands up for its existence, and avers that he has seen well-marked examples of it in Southern and Eastern Europe. With respect to the scepticism about the frequency of its appearance in former times compared with the present, the author justly condemns the fallacy which assumes that because it is now rarely met with, it has never existed. While on this subject, we may notice an interesting remark of his upon the influence which peculiarities of diet seem to possess in modifying fever.

“I have myself formed the notion, but I in no way insist on it as a well-grounded hypothesis, that the sthenic or synochal types of fever are in the present day, and perhaps have always been, most remarkably developed amongst the graminivorous and herbivorous races of men; while the putrid or typhus types of febrile action more readily develop themselves in the races amongst which animal food constitutes a large part of their ordinary aliment.”

If subsequent observation should prove the above conjecture well founded, and the geographical limits of typhus give it an air of probability, it would afford no small triumph to the vegetarians. Does it, however, we would suggest, consist with the spread of typhus among the peasantry of Ireland with whom a vegetable diet has always so widely prevailed?

We have read with great pleasure the judicious and practical observations which are made at the close of the chapter on inflammatory fever, and which show the reflecting physician. That plan of treatment which has been based on a powerful action upon some *one* system as a means of eliminating a disease extending through *every* system, is noticed by him only to be strongly condemned. The violent purgation once so common, while the Hamiltonian system was in vogue, has been found to set the fever astray from its natural course, and to produce a train of abdominal mischief. Excessive diaphoresis has been observed by the author to be similarly injurious in some cases, being followed by suppression of urine. Excessive depletion, general or local, is equally pernicious; and among the forms of bleeding occasionally required in fever, he unequivocally condemns the section of the temporal artery, for seemingly valid reasons. He objects to all exclusive modes of treatment, as repugnant to common sense; so that the system which meets his approval is the eclectic, no doubt, the most suited to fevers. Unfortunately, eclecticism is not always in the same favour with the public as with the profession,—particular systems, how-

ever incongruous, being held to be proofs of consummate genius by the former. Some one has said that "Eclecticism was like a coalition ministry," from which no good was to be expected, because there must be more or less compromise of individual opinions. However, in fever-therapeutics, except in the periodical forms of that disease, we possess as yet no specific either for their arrest or cure.

Relapsing fever has, we think scarcely met with adequate representation in the work before us, not more than half a dozen pages being specially devoted to it. In our former volumes, however, we have such extensive reports upon this form of fever, that we regard its clinical history as almost complete. The author takes exception to the generally received statement, "that relapsing fever constituted the large majority of the cases of famine fevers of Ireland," his experience in the late epidemic being "that the maculated typhus was the disease which chiefly prevailed, while the relapsing fever presented itself only at the close of the great typhus visitation." He argues that the great decennial mortality of 200,000 is too large to be accounted for by a fever so little fatal as the ordinary kind. To admit the force of the latter argument, we would require to know the numbers attacked with fever within the time, as the recoveries might have been to a large amount from relapsing fever, and the deaths from the severer forms. The opinion itself can only be affirmed or denied by the united experience of our medical brethren throughout Ireland.

The chapter on typhus commences with the adoption of the term typhosis, as an imaginary head, to which five forms are referred, as follows:—1. Typhus proper, comprising the spotted, petechial, malignant, and gaol or camp fever, and the Irish typhus *par excellence*. 2. Typhoid, or enteric, or dothin-enterite, otherwise known as the Pythogenic,\* and sometimes as the French or Continental fever. 3. Typhoid bronchitis, pneumonia, &c. 4. Cholera typhoid. 5. Traumatic typhoid. Of these five, the two first present the characters of independent fevers; whereas the pyrexial state in the three last seems to take its origin from localised disease.

From the observations of the author, it would seem that he

\* This term needs explanation, and, in another part of the book, it is defined as that form of fever which is produced from animal decomposition. It is there derived from *πυθων*, rotting, and *γίγνομαι*, I beget. Whoever invented this word did not observe the genius of the language, for compounds of two verbs are not common in Greek. Further, as *διογενής* means sprung from Jove, so *πυθωγενής* should mean sprung from Python. To be correct, it should be Pythontogenic: or better from *φθιρος* Phthitogenic, sprung from decayed things.

considered the line of demarcation between typhus and typhoid fevers to be sufficiently trenchant:—"We shall recognise a practical, clinical difference in the course, duration, symptoms, and secondary lesions of the two morbid processes." He admits also a diversity of causation,—typhus arising more from a depressed state of the system, as the effect of deficient nourishment; typhoid, from the poisonous agencies connected with decomposing matter. If these characteristics be sufficiently definite, the two diseases are not mere varieties of a single species, modified by the influence of climate, diet, occupation, &c.; but two diseases, as distinct as, in the genus *canidæ*, the species *canis* would be from the species *lupus*. There is no author who has drawn the line more decidedly than Dr. Jenner; yet, while we admit the force of the leading differentia, we think that he has not strengthened his case by the number of minute shades which he has contrasted in tabular columns.

The author's account of the symptoms of typhus is clear and full. Amongst them he has noticed an apposite name applied to the shrunken and shrivelled tongue of typhus by our volatile neighbours, as "*la langue perroquet*," or parrot's tongue. We observe a remark upon the influence of civilization on typhus, which agrees with our experience—namely, that in the higher and middle classes of society it is fatal by cerebral complication, from the great strain of mind called forth in the battle for the prizes of life; while in the humbler classes it arrives at the same fatal issue by pulmonary complication.

In speaking of the state of the circulation in typhus, Dr. Lyons has noticed more pointedly than we recollect to have seen elsewhere a singular want of harmony among the parts of the arterial circulation in certain cases:—

"We will sometimes find the carotids acting with great violence, while the radial pulse is not sensibly disturbed; in other cases, the temporal vessels will be those acting with excessive force; but the most singular state is that in which, with moderate force and volume of the heart and radial pulse, the abdominal aorta or the iliac arteries, or both, are felt to act with inordinate vigour. There is nothing more striking in the course of fever than the manner in which the several parts of the vascular system seem to act with almost perfect independence of each other."

The author's directions upon the medical treatment of typhus are so comprehensive and important, that we would not do him justice by giving them in a curtailed form. We must make an exception to this, by quoting his remarks upon the use of turpentine in the suffocation of typhoid bronchitis:—

"Turpentine constitutes a remedy of extraordinary efficacy in many cases of typhous bronchitis, when almost at the last gasp. In doses of from 15 to 20 drops, spirit of turpentine is a powerful expectorant; in extreme cases, it may be employed in drachm doses. It may be given in almond mixture, with the addition of a drachm or two of chloric ether to the eight-ounce bottle . . . It seems to act as a specific irritant upon the bronchial membrane, inducing cough and expulsive contraction of the semiparalytic circular and longitudinal muscular fibres. . . The effects produced in a few hours in these cases is perfectly astonishing."

In the general treatment of typhus, the author expresses his marked distrust of "the starvation system," as being based on an erroneous pathology; and he quotes from the touching and appropriate eulogy pronounced upon the late Dr. Graves by his eminent colleague, a saying of the former, that when he died, the most honourable memorial which he would wish to have inscribed on his tomb was, "that he had fed fevers." There is little doubt that the opposition to the administration of sustenance had been carried too far, and that since Dr. Graves drew attention to the fact, a salutary reaction has occurred. As a disciple of the Meath Hospital, we should look to the author for kindred views. Accordingly, he enters fully into the regimen of the typhus patient, and even gives a bill of fare for the day, which certainly cannot be accused of stinginess. Thus, instead of maintaining a perpetual current through his patient's *primæ viæ* according to the received notions, "by physic and slops," to his great discomfort, he gives him for breakfast, tea or coffee and toast; for lunch, a biscuit and a couple of glasses of sound wine; for dinner, good strong beef-tea and toast again, or a little roast or boiled, if the patient desire it; an unexciting supper of arrow-root, at eight o'clock, follows; with a couple more glasses of sound and generous wine, as a nightcap, to close the day. This is certainly an improvement upon the washing and scrubbing system aforesaid, and decidedly much more palatable to the patient himself. The latter, we take it, will not be slow to award, with M. Argan, a well-deserved compliment to his doctor, in which we heartily join:—"Ce qui me plait de Monsieur, mon médecin, c'est que ses parties sont toujours fort civiles."

We had marked some passages in the chapter on typhoid fever, such as the broad line of distinction to be observed in its treatment from that of typhus—the more guarded use of stimulants—the value of quina—and the careful management of perforation of the intestines; but we must desist—

"Nam mihi commotâ jamdudum mulio virgâ  
Innuît."

We cannot, however, dismiss the present work without observing that it needs no laboured panegyric from us; it is itself its best praise. It embodies the results of great and untiring industry, extensive knowledge, and critical discernment; and is creditable to the school of medicine in which the accomplished author received his education. It would be unfair to the publisher not to acknowledge the way it is brought out. To the zealous student of febrile diseases, we say, go, buy it, and judge for yourself; when you have mastered its contents, you will possess no mean knowledge of fever.

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*A Year Book of Medicine, Surgery, and their allied Sciences, for 1859.* Edited by DR. HARLEY, DR. HANDFIELD JONES, MR. HULKE, DR. GRAILY HEWITT, and DR. ODLING, for the New Sydenham Society. London: 1860. 8vo. pp. 536.

THIS publication of the New Sydenham Society is little more than a "Catalogue raisonnée," and yet as such is neither accurate nor complete, being replete with errors, typographical and other, and incomplete, inasmuch as it omits all mention of many very important papers published within the period it professes to embrace.

The council of the society have already announced that their year book for 1860 is to be on an improved plan, and to give much more matter. That it may be greatly improved, cannot be doubted; but we can and do doubt very much the advisability of their publishing such a book at all: certainly, in its present form, we think it must be valueless to all the members of the society, except those few who are engaged more in literary pursuits than in the practice of their profession; and even to them it can only serve as an index, directing them to where they may find papers on the subjects they may have in hands, for the abstracts are neither complete enough, nor clear enough, to be themselves depended on. The abstracts from papers of practical interest are often so short and sketchy, as to be more likely to lead their readers astray than otherwise.

This system of republishing abstracts of all the important papers that appear from time to time in the various periodicals is one of very doubtful propriety and prudence; it is most injurious to the journals, interfering with their circulation and legitimate sources of support; it is like killing the goose that lays the golden eggs, and cannot fail ultimately to be injurious to the science and profession of medicine.



These year books, abstracts, and retrospects, engender a habit of desultory and unimproving reading, whereby loose habits of thought are created, or rather thinking is prevented; and as the ploughman idly whistles for want of thought, so the reader of such productions turns page after page, and thinks never. Let the Sydenham Society continue to publish such admirable monographs as they have hitherto done, and they will thus insure their own success, further the progress of our science, and serve the best interests of their subscribers.

*On Myalgia: its Nature, Causes, and Treatment; being a Treatise on painful and other Affections of the Muscular System, which have been frequently mistaken for Hysterical, Inflammatory, Hepatic, Uterine, Nervous, Spinal, or other Diseases.*  
By THOMAS INMAN, M. D., Lecturer on Practice of Medicine, Liverpool Royal Infirmary School of Medicine, &c.  
London: Churchill. 1860. 8vo, pp. 307.

THE author of the work whose title stands at the head of this article is evidently one of that energetic and untiring class of men, who, having taken up a subject in right earnest, appear to be inspired with the determination of working it to the utmost, until at length their mind becomes so thoroughly absorbed in their favourite topic, that, gradually and by insensible shades, a number of affections formerly supposed to be well understood, and to be easily referable to their true positions in the nosology, are one by one constrained to arrange themselves in the favoured category. Old and familiar phases of disease assume a new aspect, and observers of well-known facts arrive at the facile capability of investing said facts with an interest at once novel and startling, because necessarily at variance, in a greater or less degree, with preconceived or established notions regarding their interpretation. The very title of the present work would indicate that the views of the author have undergone some degree of modification within a few years; for, from considering at first that he had to deal with a number of simple cases which could readily be recognised as being purely muscular in their origin, he described them under the title of "Certain Painful Muscular Affections" in 1856; but on finding that the information he had gained was capable of a wider application, and threw much light on the mysterious subject of spinal irritation, he published, in 1858, an extension of the former work, under the title of "Spinal Irritation explained." this latter was reviewed in our 26th volume. "A more ex-

tended experience," he says, "proved that the subject had far wider ramifications than were at first dreamed of." The author has met with a great number of instances in which patients have been under treatment for months for a supposed *uterine* disease, while in reality they have been suffering from pain in the pubic insertion of the rectus abdominis. He has been able to show that a great number of those pains habitually spoken of as *hysterical*, have no real connexion with that disease; and that the very signs relied on were, in reality, proofs that the symptoms were of a purely muscular origin. He was further enabled to throw considerable light upon some of the prominent symptoms of *pleurisy* and *peritonitis*, and to show that many cases called by the latter name were purely myalgic in their nature. This being so [?], it was clear that the subject of muscular pain had as important a bearing upon hysterical, inflammatory, and uterine diseases, as it had upon spinal affections; and to designate his work simply "an explanation of spinal irritation" would imperfectly describe its tendency.

The *naïveté* with which the author describes the difficulty of selecting an adequate title, and how he got over it, tempts us to transcribe the next paragraph. It appears that he "was guided to the choice of the word 'myalgia,'—first, because it had a familiar look about it, as resembling neuralgia; secondly, because it implied no other theory than that the muscles were the seats of pain; thirdly, because it had a more classical sound than the more homely phrase 'muscular pain,' which, by its frequent repetition, would suggest to some facetious patient to dub the doctor by the ignominious title of muscle-man." It may not, perhaps, have occurred to him that, albeit the term is orthographically correct, and profoundly classical as to sound, it is nevertheless suggestive of the notion that although the sundry pains and aches which have a special seat have long since been designated by terms ending in *algia*, these terms are no longer the conventional property of practitioners, but have passed (thanks to the diffusion of knowledge) into colloquial use among patients; and that while he has ingeniously succeeded in anticipating the Mahometan allusion of the supposed facetious patient, some equally thoughtless *bel-esprit* may recognise in the newly coined term, a simple appropriation on the part of the doctor, who would fain designate the fashionable ailment by the name of *my algia* to distinguish it from the *algias* of other men.

In order that our readers may be enabled to form an idea of the general drift of the present work, it may be stated that the author endeavours to show, by a lengthened series of ob-

servations, illustrated throughout by the details of numerous cases—1. That the symptoms attributable to “spinal irritation” have nothing to do with the spinal cord, or the nerves arising from it. 2. That the majority, if not the whole of them, are due essentially to a cause similar to that which produces the spinal tenderness. 3. That the spinal tenderness results from over-straining of the fibrous origins of the muscles attached to the spinous processes. 4. That the spinal tenderness is analogous to that experienced at the origin and insertion of muscles in other parts of the body. 5. That the weaker the individual is, the greater is the tendency to fibrous pain. 6. That the most common causes of the pain and tenderness in any part of the muscles, are constitutional or acquired debility, coupled with too great an amount of work. Therefore, as soon as the diagnosis of muscular pain is made, the next inquiry should be into the causes which have produced the debility, supposing that no extra exertion has been made. 7. That debility increases equally the irritability of the muscular and the nervous systems. 8. That before hysteria can manifest its presence, there must be debility from some cause or other. 9. That that debility may show itself in the muscular or nervous system, or both. 10. That debility affects the nervous system as a whole or in sections—that is, mental, sensitive, motor, organic. 11. That functional affections in any one or more of these parts have long been recognised as emanating from deficient vital power. 12. That anything which deteriorates the vital power has a direct tendency to aggravate the complaints referred to. 13. That muscular and nervous irritability are subject to the same laws, and that the remarks applicable to the one are *mutatis mutandis*, applicable to the other. 14. That many symptoms hitherto supposed to be due to hysteria, spinal irritation, or nervous irritability, to pleurisy, peritonitis, uterine, or stomachic disease, are essentially muscular in their origin, and are produced by fatigue in enfeebled constitutions, and that the general opinions respecting the diagnosis of hysterical symptoms require a complete remodelling. 15. That the link connecting hysteria with spinal disorders is constitutional or acquired debility. 16. That, as regards curious mental phenomena, excess of sensibility in the nerves of common or special sensation, or a propensity to spasmodic actions, and to irregular organic phenomena, there is no essential distinction; they are simply different facets of the same die. 17. That the essential distinction between genuine hysterical and muscular affections is, that a large amount of bodily rest is necessary for the cure of the

latter, while it is not so absolutely requisite for the former. That the former are difficult, the latter comparatively easy of cure. 18. That for the future, it will be necessary to discriminate between pain arising from muscular fatigue, cramp, or fibrous stretching, genuine neuralgia, and hysteria; and that there will be neither precision in diction nor clear idea of treatment until the distinction is made.

From the foregoing recapitulation of the author's views regarding the nature of myalgic suffering, the reader will find but little difficulty in deducing the principles of treatment suitable to the various cases in which muscular pain constitutes a prominent or essential feature. The importance of observing such rules as relate to the economising of a patient's strength, and of promoting its increase by suitable measures; the value of change of air or of occupation, as well as the necessity of directing a generous and nutritious diet, with such tonics as the particular cases may require; all this is copiously treated of towards the latter part of the volume; indeed, in this, as in every portion into which the work is divided, the minuteness with which the most ordinary and commonplace particulars are dwelt upon is, in our opinion, better adapted for the listless perusal of some *malade imaginaire* than calculated to meet the healthy requirements of the busy and hurried practitioner, who would be content with a bold and broad outline of facts and principles, trusting to his own general knowledge and experience for filling in the details. Before we conclude, we must extract a passage from the chapter on the *diagnosis* of muscular pains, nor is it the only place in which a case adduced "in illustration" appears to us to have been rather inapposite, and capable of an interpretation somewhat different from that which the author would suggest:—

"When the pain is a fixed one, it almost invariably occupies a spot which is more or less tendinous, or where the muscles are attached to the bones; and this should at once enable us to distinguish it from neuralgia. There is, however, one exception to this point of diagnosis, to which I must refer. A woman cannot be expected to point to the pubic insertion of the rectus,—she always points to the hypogastric region instead; but, as far as my experience goes, the practitioner will have no difficulty in gaining the information he requires by indicating in his own person [!] the precise seat of pain. I find, that many women suffer pain at the origin of the lesser pectoral, which they do not tell of, lest it should necessitate an examination of the mammæ. One patient, who described all her other symptoms fairly, omitted this; but when I questioned her, she answered that her breasts were often so acutely tender that she could not bear to touch them. She was a very stout,

florid-looking girl, with abundance of energy and spirit. The cause of the debility and muscular exertion was, in her case, excessive vomiting. I suspected pregnancy, and heard that she subsequently aborted."

Leaving our readers to make such comments upon this case as their several notions thereof may suggest, we proceed, in conclusion, to notice what we conceive to be by far the best part of the book, namely, the lithographic illustrations. The delicate finish of their execution is in the highest degree creditable to the artistic skill of Mr. Bagg; and with the exception of the *pose* of the first figure, which is not quite conformable to correct rule, the most critical can find no fault in the accuracy of the designs which that artist has so graphically portrayed. What we are disposed to object to is, the indelicate misemployment of the drapery held in the right hand of the young lady who faces the title page; were this figure *completely* nude, the "model" might plead the very natural excuse of "nothing to wear;" but as this is not the case, the artist would have consulted grace, as well as delicacy, had he contrived a more judicious disposal of the drapery, without encroaching upon even the inferior boundary of the myalgic landmarks.

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*Course of Lectures on the Physiology and Pathology of the Central Nervous System*, delivered at the Royal College of Surgeons of England in May, 1858. By C. E. BROWN-SÉQUARD, M.D., &c. London: Williams and Norgate, 1860. 8vo, pp. 118.

THIS is a reprint of the lectures delivered by Brown-Séquard, at the College of Surgeons, London, in 1858, which contain, the author says, the results of the work of almost all his life, since he began to study medicine. With the modesty of true genius he invites criticism, acknowledging that as the questions discussed are as difficult as they are important, and as many of them are quite new, so that he could not have the benefit of the views of other authors to guide him in their examination, he may have come to erroneous conclusions on many points. It is in the power, however, he says, of most medical practitioners to prove or disprove his views; for out of the millions of patients yearly treated by those who may peruse the book, there are indeed many thousands whose cases may throw light, and often a decisive light on the questions discussed.

And as most of these questions are of the greatest importance, in both a practical and a scientific point of view, he expresses a hope, in which we cordially unite, that those who will peruse the book will not let the cases pass unrecorded that may be the means of settling what is yet undecided in the matters considered.

But the criticism invited is the criticism of facts, the daily observations of patient clinical observers: if these confirm the doctrines here put forth, they will establish them on an unassailable foundation; but if the facts derived from the more extended sphere of observation require the theories to be abandoned or modified, we are confident that none will be more ready to do so than the author himself. We propose here simply to state the doctrines deduced by Brown-Séquard from his researches, with a view to direct attention to them, and to recommend all interested in the advancement of medical science, and all anxious to clearly understand the functions of the spinal cord, and the diseases to which it is liable, to study the lectures for themselves, which are now published for the first time in a separate form. The lectures as delivered were only six in number, but they are now extended to twelve, being swelled out by the addition of a great variety of illustrative cases and pathological observations; an appendix is added, containing additional facts, and an examination of the objections that might be raised to the doctrines propounded, and of the principles of treatment that might be deduced from the theories established.

The first lecture opens with a statement of the mode of investigation adopted; it is shown that while no means that science may furnish should be neglected, the results of experimentation upon living animals, and observation of pathological cases, when taken together and compared, are most worthy of reliance. We have then some observations on the functions of the anterior and posterior roots of the spinal nerves, and some new arguments adduced as to the truth of Sir Charles Bell's statement that these preside respectively over motion and sensation from the mode of decussation of their conductors. The "Recurrent Sensibility" of Majendie is traced to its true cause. Majendie discovered, as is well known, that irritation of the anterior roots caused pain, but in a less degree than the posterior roots. To explain this, it was suggested that a branch of nerve leaves the posterior root where it unites with the anterior, and returns in the anterior to join the spinal cord, and such a nerve has been actually portrayed in some of our elementary works on physiology. The author, however, shows

that the signs of pain observed by Majendie really arise from impressions made on the peripheral extremities of the fibres of the posterior roots that are distributed to the muscles that are thrown into contraction by the irritation of the anterior roots. He refers to the experiments of Matteucci, as showing that each time a muscle contracts, some change in its galvanic condition is effected. This change affects the nerves of sensation, and hence the signs of pain discovered by Majendie. He shows, moreover, by an experiment of his own, that it is the tendency to contract that causes the pain, rather than the actual shortening of the muscular fibre, and that the greater the resistance to the shortening of the muscle, the greater the pain, and in this way he accounts for the pain of various muscular affections.

"If we compare these results with the following pathological facts, we find that the phenomena are much alike in the two series of facts that we compare, and, therefore, that they seem to depend on the same causes. I suppose a case of painful contracture of the anterior muscles of the thigh; the pain is increased very much every time the contracted muscles are elongated, *i. e.*, when the resistance to the contraction is augmented; on the contrary, it diminishes when the resistance to the contraction is rendered less than it was, and, at last, *it disappears entirely, or almost entirely, when the resistance is completely, or almost completely, destroyed*, after tenotomy. Surgeons, till our researches, had not been able to explain this apparently strange cessation of pain; now it seems quite simple to understand that such should be the case.

"In cases of *fissura in ano*, it is very well known that the pain due to the spasm of the sphincter is increased when there is a resistance to the contraction, and that the greater the elongation of the muscular fibres, the greater also the resistance to their contraction and the degree of pain. At last, when the muscular fibres can contract freely, and almost without resistance, the pain disappears, as is the case after the operation of Boyer (the section of the sphincter). Of course the pain depending upon the fissure persists, but that due to the muscular spasm disappears. In this case, also, surgeons could not explain the cessation of pain. We find here, as in the preceding case, that the excitation of the nerves due to the muscular contraction augments, decreases, and disappears in the same circumstances, concerning the degree or the absence of resistance, which produce analogous phenomena in the experiments above mentioned.

"In cases of neuralgia, when the sensibility of nerves in muscles is increased, there is pain produced or increased every time the muscles contract.

"The contractions of the uterus, which are the more painful the more there is resistance opposed to them, cause also pain in the same way as the spasm of the anus or the contraction of the mus-

cles of the thigh. The relation between the degree of contraction of the uterus and that of pain is so evident, that the word 'pains' is employed for that of 'contraction.'

"Every muscular contraction seems to generate a galvanic excitation of the sensitive nerves in the neighbourhood of the muscular fibres; and, the degree of excitation being in proportion to the degree of energy of the contraction, we have, in this way, an excellent means of judging of the state of the muscles. When the contraction acquires the degree which it has in cramps, then it causes pain, which is also in proportion to the energy of the spasm. If we succeed when we have a cramp in making it cease by elongating the contracted muscle, we find that the pain often increases at first, and disappears only when the contraction has ceased. I regret not to be able to bring forward all the reasons which have led me to admit the views I have just proposed, but I must keep within my programme.

"If, now, we examine what takes place in the apparently paradoxical experiment of Magendie, we find that nothing is more easily explained. When the anterior roots of a spinal nerve are excited, a cramp, is produced in the muscles in which the nerve-fibres of the roots are distributed, and the pain which belongs to a *cramp* is generated. I think this pain is due, as I have said for cramps, to a galvanic excitation of the sensitive nerve-fibres existing in the muscles which contract; but whether this theory be true or not is, in a measure, indifferent as regards the general cause of pain in the experiment of Magendie. In fact, there is then the same cause which exists in a cramp; and this cannot fail to be so inasmuch as a real cramp is generated by the irritation of the anterior roots. So, then, we can conclude: 1st, that the *recurrent* sensation is only in appearance recurrent; 2d, that the anterior roots of the spinal nerves cause pain when they are irritated, because they produce a cramp: 3d, that, consequently, *there is no sensibility of any kind in the anterior roots, and that it is because they are motor, and not because they are sensitive, that they cause pain when they are irritated.*

"Therefore, the objection which has been urged against the views of Sir Charles Bell, and which was founded upon the fact that the anterior roots cause pain when irritated, is unfounded."

We have next considered the statement of J. W. Arnold, that the nerve-fibres that convey the knowledge to the sensorium of the state of the muscles are contained in the anterior roots; and while it is admitted as very probable from the facts referred to by Arnold, that some such fibres may pass up through the anterior roots, yet it is shown that it is not thus only that the brain derives a knowledge of the state of the muscles, and that certainly all fibres that convey a sense of pain and a sense of touch pass through the posterior roots.

But while the truth of Sir Charles Bell's discoveries as to



the functions of the roots of the spinal nerves is thus demonstrated, it is shown that his ideas as to the functions of the columns of the spinal cord were incorrect. It is well known by a generalisation, perhaps natural, but very hasty, Sir Charles inferred that the posterior columns of the spinal cord were the continuation of the posterior roots of the spinal nerves, and presided, like them, over sensation, and the anterior columns over voluntary motion. This theory he supported by a very imperfect experiment; it was opposed, however, by many eminent physiologists; and at length Sir Charles, finding, as he says himself, that the theory barred his progress, renounced it in a paper read before the Royal Society in April, 1835, adopting instead the opinion that the lateral columns must be the parts transmitting sensitive impressions.

Notwithstanding, or perhaps unaware, that Sir Charles had abandoned his first theory on this subject, Longuet, a French physiologist advocated it so ardently in 1842, that it was soon very generally received, and even taught, in some of our class-books. Todd and Bowman, however, assailed it in their valuable work on physiological anatomy; and Todd gave it the *coup de grace* in his most able and comprehensive article on the physiology of the nervous system in the *Cyclopædia of Anatomy*.

Brown-Séquard devotes his second lecture to the consideration of the functions of the posterior columns. He refers to the anatomical researches of Todd, Lockhart Clarke, and others, as showing that the posterior roots are not connected with the posterior columns of the spinal cord. He criticises the statements of Longuet, showing that they are so self-contradictory as to be untenable, and that he overlooked important elementary principles in his experiments, and then proceeds to develop his own views, and explain the experiments on which they are founded. For these we must refer our readers to the work itself, as we cannot do more than briefly summarize them here. First, it is shown that division of the posterior columns, instead of depriving all parts of the body behind the section of sensation is followed by marked hyperæsthesia, the rationale of which is to be explained hereafter. 2ndly, it is found that division of all the cord, except the posterior columns, destroys sensibility. 3dly, Dr. Brown-Séquard shows that the lateral columns do not convey sensibility (the theory adopted by Sir C. Bell, when he found that the posterior columns did not perform this duty). He shows this by an experiment in which he divided the lateral columns; when sensation, instead of being lost, was increased, as in the case of the posterior. 4thly, he shows that the grey matter is the principal conductor of sensations in the

spinal cord, the anterior columns assisting in a slight degree. 5thly, that it is the central part of the grey matter that conducts the impressions—that the anterior horns may assist in this, but that the posterior take no part in it. 6thly, that the grey matter conveys sensations by means of the connexion of the cells with one another and the white fibres surrounding the grey substance. 7th, that the conductors of impressions either enter the central grey matter directly, or pass to it through the posterior, or posterior part of the lateral columns, or through the posterior horns of the grey matter, and in these run both upwards and downwards for a short distance before joining the grey central matter. 8thly, that the restiform bodies which are the continuations of the posterior columns do not give transmission to any of the conductors of sensitive impressions of either the various parts of the trunk and limbs or of the head; and that therefore the cerebellum, with which the restiform bodies are connected, does not receive from them any such conductors.

The third lecture contains an account of the most interesting and novel contribution, (as we believe) that Brown-Sequard has made to physiological science, viz., the fact that the conductors of sensation decussate with one another in the spinal cord—that the decussation is complete in mammals, but not so much so in birds and reptiles—and that it takes place at a short distance from the points of insertion of the posterior roots. These inferences are drawn from a series of experiments which seem to establish them completely:—

“1st. The spinal cord of a mammal is laid bare at the level of the two or three last dorsal vertebræ, and a lateral half of this organ (including the posterior, the lateral, and the anterior columns, and all the gray matter on one side) is divided transversely. The animal is left at rest for a little while, and then it is ascertained that sensibility seems to be much increased in the posterior limb on the side of the section, while it seems to be lost, or extremely diminished, in the posterior limb on the opposite side. There seems to be, therefore, *hyperæsthesia* behind and *on the side* of a transversal section of a complete lateral half of the spinal cord; while, on the contrary, there seems to be *anæsthesia* behind the section, and *on the opposite side*.

“This experiment is one of the two made by Galen; but he seems not to have looked at all at the condition of sensibility, and he simply states that there is a paralysis on the side of the section, and no paralysis on the opposite side.

“Sir Astley Cooper, under the suggestion of Dr. Yelloly, has made a similar experiment, except that the section was higher; the

state of sensibility is not mentioned, and, as regards movements, there was paralysis on the side operated upon.

"Schœps, Van Deen, and Stilling have observed that sensibility is not lost in the limb or limbs behind, and on the side of the section of a lateral half of the spinal cord; but they have not remarked the most important fact, that on the opposite side there is anæsthesia. They also do not mention this curious result of this experiment, the existence of hyperæsthesia on the side of the injury.

"Fodéra was very near discovering that there is a decussation of the sensitive fibres in the spinal cord. He says he has found in some cases, that a section of one of the posterior columns caused a diminution of sensibility in the opposite side of the body; but he states that in other cases he has seen the reverse. He also has sometimes remarked that the section of one of the posterior columns causes hyperæsthesia in the same side, and that a section of these two columns produces hyperæsthesia in the two sides, but he declares, also, that he has seen the reverse.

"Two explanations for some of the results of a section of a lateral half of the spinal cord may be proposed, as regards sensibility. Either it may be imagined, as it has been by several German physiologists, that the gray matter has the power of transmitting sensitive impressions in such a manner that one lateral half of this substance is sufficient for the two sides of the body, or that the conductors of sensitive impressions decussate in the spinal cord, so that those which come from the left side of the body pass into the right side of the spinal cord, and *vice versa*. The hypothesis of the Germans may explain the fact that sensibility persists on the side of the section, but it is proved to be absolutely inadmissible by the fact that there is anæsthesia on the opposite side. We will see that the other experiments we have to mention are also in opposition to the view of the Germans (Stilling, Schiff, and others). On the contrary, all the facts concur to prove the existence of a decussation.

"2d. If, after having made a first section of a lateral half of the spinal cord in the dorsal region, on the right side, for instance, and after having ascertained that the *right* posterior limb is hyperæsthetic, or at least extremely sensitive, we divide the *left* lateral half of the spinal cord in the cervical region, we find then that the *right* posterior limb loses entirely, or almost entirely, its sensibility. This experiment shows clearly that the sensitive impressions coming from the *right* posterior limb, after the first section, passed across the spinal cord from the right into the left side, along which they were transmitted to the encephalon.

"3d. To obtain a very striking result from the experiment which consists in only one section of a lateral half of the spinal cord, it is better to make it after the posterior columns have been divided. We know that after this division there is hyperæsthesia in the parts of the body which are behind the section; if, after having ascertained this fact, the section of a lateral half is completed where the

posterior columns have been divided, we find that the hyperæsthesia seems to increase on the side of the second operation, while, on the opposite side, not only the hyperæsthesia, but sensibility entirely disappears.

"4th. There is another mode of proving that the conductors of the sensitive impressions decussate in the spinal cord. In several points of view this mode of demonstration is superior to the preceding. It consists in a longitudinal section of the spinal cord, an experiment already made by Galen, but the results of which, as regards sensibility, have been overlooked by him.

"The spinal cord is laid bare in the whole lumbar region, and a careful division of the entire extent of the part of the organ giving origin to the nerves of the posterior limbs, is made so as to separate the two lateral halves of the organ, one from the other. If this experiment could be executed perfectly well, nothing would be divided in the cord except the commissures, which unite the right side with the left side of the cord, and all the longitudinal elements of this nervous centre would be left uninjured; but it is impossible not to cut more or less on either side. However, when the operation has succeeded well—i. e., when the two separated halves have been very little injured, a striking result is obtained. The voluntary movements still exist in the posterior limbs (though diminished on account of the injury to the muscles of the lumbar region), but *sensibility is entirely lost* in them. To those who know that injuries to the spinal cord, which cause a diminution of sensibility, always produce a greater diminution of voluntary movements, this fact will not be explained by the supposition that some injury has, then, been made to the two halves of the cord, and that it is in consequence of this supposed injury that the loss of sensibility is due. At least it will, I think, be easily admitted that if the two lateral halves of the cord had been injured enough to produce a complete and lasting anæsthesia, there would be a notable degree of paralysis of voluntary movements. We repeat that such is not the case: the animal has the use of his two limbs; he moves about pretty freely, as Galen had already said. The loss of sensibility, therefore, must depend on the section of the commissures of the spinal cord, or, in other words, on elements of this organ which cross each other in the median line, or, rather, the median plane.

"If now we compare the results of this experiment with those of a transversal section of a lateral half of the spinal cord, we find that they agree perfectly in showing that the conductors of the sensitive impressions decussate in this organ. It is useless to stop to show that the longitudinal separation of the lumbar enlargement of the spinal cord would not produce anæsthesia, if the German physiologists were right in admitting that the gray matter has the power of transmission in every direction. It would be useless, also, to insist upon the disagreement between the results of a longitudinal section of the spinal cord, and the views of the physiologists who admit that the posterior columns are composed of sensi-

tive fibres, coming from the posterior roots, and going up to the encephalon. These columns are left almost entire and uninjured, and nevertheless, sensibility is lost.

"5th. Another experiment, which is a combination of two of the preceding, gives a still better proof of the decussation of the conductors of sensitive impressions in the spinal cord. A longitudinal section is made on the cervico-brachial enlargement of the spinal cord, so as to separate it in two lateral halves. I ascertain then that sensibility is lost in the two anterior limbs, while it remains, and even seems to be increased, in the two posterior limbs. Of course, if the loss of sensibility in the two anterior limbs depended upon an injury to the two sides of the cord, and not upon a section of the decussating conductors of sensitive impressions, there would be a loss of sensibility, or at least, a diminution of it in the posterior limbs. The admission of a decussation explains the two facts: loss of sensibility in one set of limbs, and conservation in the other set. If we divide transversely, in the same animal, the right lateral half of the spinal cord, we find then that the posterior limb on the same side becomes more evidently hyperæsthetic than before, and that the left posterior limb loses its sensibility. The transmission for this last limb therefore took place by the right half of the cord, while that for the right posterior limb continues to take place by the left half of the cord."

Lecture iv. opens with asserting that the senses of touch, pain, temperature, and of muscular contraction, have each of them separate conductors, and proceeds to show that these conductors do not proceed to the brain along the posterior columns, notwithstanding the assertion of Moritz Schiff that the conductors of tactile impressions do pass along these columns; and it is also shown that all these conductors decussate, as already described. The lecturer then asks how is it that sensibility is not lost, and is only more or less diminished, although the spinal cord is deeply affected? In reply, he shows that the conductors of sensation are diffused throughout the whole substance of the cord except the posterior columns, a view which explains the so frequent persistence of sensibility in cases of disease of the spinal cord:—

"If, for instance, we imagine that there are a thousand conducting elements coming from a small part of the right side of the body, in the left half of the spinal cord, they are scattered in all the parts of the conducting zone of this half, so that to divide them all, a section must divide the whole of this zone. In other words, we can say, that *every small portion of the conducting zone in a lateral half of the spinal cord contains conductors of sensitive impressions coming from all the points of the body on the opposite side, which are behind the place of this small portion.* We can say, also, that the sensitive

*impressions made on any point of the lateral half of the body are transmitted to the sensorium by conducting elements, distributed in all the parts of the lateral half of the spinal cord on the opposite side.*

"This view, which explains the so frequent persistence of sensibility in cases of disease of the spinal cord, is entirely different from that of Stilling and others who admit that a part of gray matter in one half of the cord is sufficient for the transmission of sensitive impressions from both sides of the body."

The author now passes to the consideration of the transmission of the orders of the will to the muscles through the spinal cord. He finds it is very much more difficult to determine what are the parts of this organ employed in voluntary movements than to find out what are those through which the sensitive impressions are transmitted. The results on this point at which he arrives are, that in the dorsal region all the various parts of the spinal cord, except the posterior columns, are employed in the conveyance of the orders of the will to muscles—that in this region the *anterior* columns and the grey matter are the most important; and of the grey matter that in the anterior half of the cord, in the conveyance of the orders of the will; but near the medulla oblongata he finds the *lateral* columns and the grey matter between them the most important. In some animals there are reasons to believe a decussation of the volitional fibres takes place to some extent in the spinal cord; but pathological facts do not bear this out as occurring in man, in whom the crossing of the anterior pyramids in the medulla oblongata is the only decussation that takes place. The lecture concludes by showing that the results of experiments afford no support to the assertion of Bellingeri and Valentin that the motor fibres that go to the extensor muscles pass through the posterior columns, while those that go to the flexor muscles go through the anterior columns.

Lectures v., vi., vii., and viii. are taken up with the examination of pathological cases, in which the symptoms have been carefully observed during life, and the morbid appearances faithfully recorded. Some of these had been observed by the author himself; but the greater part have been published by various physicians in all parts of the world, a circumstance adding not a little to their value. They fully bear out the principles laid down in the previous lectures, and we would earnestly recommend their careful study to all our readers. The first group of cases shows clearly that the posterior columns may be deeply altered; and yet, sensation in the limbs be perfect or even exaggerated, proving that they are not the sensory

tracts, and the same is proved of the restiform bodies. The next group shows that the grey substance is the principal channel for the transmission of sensitive impressions. We have then two series of cases proving the decussation of the conductors of sensation. The first series of these are summed up as follows:—

“Setting aside, in this lecture, what relates to temperature, we state that a transversal section of a lateral half of the spinal cord causes a loss of voluntary movement in the corresponding side of the body, and a loss of sensibility in the opposite side, and if, now, we look at the cases we have related to see how they agree with this general result, we find the following points:—

Cases.	Side of the injury.	Side of the paralysis.	Side of the anæsthesia.	Side of conservation of sensibility, or of hyperæsthesia.
29	<i>right</i>	<i>right</i>	<i>left</i>	<i>right</i>
30	<i>left</i>	<i>left</i>	<i>right</i>	<i>left</i>
31	<i>right</i>	<i>right</i>	<i>left</i>	<i>right</i>
32	<i>left (probably)</i>	<i>left</i>	<i>right</i>	<i>left</i>
34	<i>right (ib.)</i>	<i>right</i>	<i>left</i>	<i>right</i>
35	<i>left (chiefly)</i>	<i>left (chiefly)</i>	<i>right</i>	<i>left</i>

“If we add to these cases those in which we have no other reason but the analogy of symptoms with those observed in animals, such as Cases 33 and 37, we have two more instances of this curious but simple morbid manifestation—i. e., loss of movement in one side, loss of sensibility in the other. In animals, as I have often said and shown, the paralysed parts are in a state of hyperæsthesia; in the cases I have related, hyperæsthesia has been noted in Cases 32, 33, 35, and 37. It was so great that the least touch produced pain in Cases 32 and 35. It would have been found in the other cases, had the physicians who attended the patients looked for it.”

The eighth lecture reviews the conclusions to be drawn from the cases detailed in the preceding lectures. It is shown that these cases prove that the posterior columns are not the channels for the transmission of the orders of the will to the voluntary muscles; and that paralysis of voluntary motion, is not a symptom of section or local destruction of these columns. It is also shown that the clinical facts are in opposition to the theory of Bellingeri, that the posterior columns supply the extensor muscles. Spasm of the flexor muscles of the lower limbs—spasm so powerful, that the thighs come almost in contact with the abdomen, while the heels are drawn up so as to touch the back parts of the thighs—is a symptom that seems to belong exclusively to disease of the spinal cord, but does not belong exclusively to disease of either the anterior or posterior columns, as regarded by Bellingeri's theory.

On reviewing all that is known of alteration of the posterior columns, it is found that, when limited to a small extent, it does not affect voluntary movements; but when the alteration extends to a few inches in either the cervical or dorso-lumbar regions, these movements are always more or less impaired; that when the cervical region alone is affected, the upper limbs are paralysed, and not the lower, except from other causes. Destruction of the whole length of the posterior columns is said to be found with complete paralysis; but, on examining the cases, Brown-Séquard concludes that the paralysis was due to other parts of the cord being affected. Alterations of limited portions of the posterior columns do not interfere with reflex movements; but, if extensive, prevent these movements and so enfeeble voluntary ones. If the entire length and thickness of these columns in the lumbar region be affected, the patient is unable to stand or walk because of the loss of reflex actions, but when lying in bed is able to move the limbs pretty freely.

The *lateral* columns of the spinal cord, near their decussation, are the channels of volition, with, perhaps, some fibres in the grey substance; destruction of them here causes paralysis, while that of the *anterior* columns at this point does not; but alteration of these anterior ones, lower down, is always followed by loss of voluntary motion, and this symptom exists also in alterations of the grey matter. Paralysis of voluntary motion alone, then, cannot be of service in the diagnosis of the place altered in the spinal cord; but the various modifications in the degree, in the extent of place, and in the kind of paralysis, and the co-existence of this symptom with others, &c., are able to guide more or less surely in the diagnosis of the seat, and also of the nature of the alteration in the spinal cord.

The lecturer next passes to the consideration of anæsthesia as a symptom of disease of the spinal cord. He finds that it is not connected with disease of the posterior columns, but rather with that of the grey substance. There are, he says, many pathological cases which can only be explained on the hypothesis, that there are different conducting fibres for the several kinds of sensation, as—1. Touch; 2. Tickling; 3. Pain; 4. Heat; 5. Cold; and that these enter different parts of the spinal cord. He further shows that injury confined to any of the white columns does not cause loss of sensation; but that if the lateral half of the cord be affected, there is anæsthesia of the opposite side of the body. The lecture terminates with the following enunciation of the groups of symptoms existing in the cases of disease of the spinal cord



according to the place and extent of the alteration. He has, however, omitted purposely the symptoms concerning the movements of the heart, respiration, the state of the sphincters, animal heat, and nutrition:—

“1st. *Deep alteration of the posterior columns in all their length.*—Increased sensibility in the trunk and limbs for impressions of touch, or due to pricking, pinching, and galvanic excitations, and for changes of temperature (cold and heat). Loss, or a very great diminution, of reflex movements. All kinds of voluntary movements possible, and more or less easily executed when the patient is in bed. Walking and standing very difficult.

“2d. *Deep alteration of the posterior columns in the extent of the cervico-brachial swelling.*—Increased sensibility in the four limbs, and in the trunk, for all kinds of impressions. Diminution of reflex actions in the upper limbs, and increased reflex actions in the lower limbs. Some difficulty in the direction of the movements of the upper limbs, without the help of the sight. Standing and walking possible without any great difficulty.

“3d. *Deep alteration of the posterior columns in the extent of the dorso-lumbar swelling.*—Increased sensibility in the lower limbs, and normal sensibility in the upper ones. Diminution or loss of reflex actions in the lower limbs. Movements of lower limbs possible, and even easy, when the patient is in bed; but walking and standing very difficult.

“4th. *Deep alteration of a very limited part of the posterior columns.*—Increased sensibility, and increased reflex action, in all parts receiving their nerves from the spinal cord below the alteration. Voluntary movements possible, and even easy, everywhere. The place of the alteration may be detected by diminution of reflex actions in the zone round the body receiving nerves from the level of the part altered in the posterior columns.

“5th. *Alteration of the posterior columns and posterior roots of the spinal nerves.*—Instead of hyperæsthesia, as in the preceding cases, diminution or loss of all kinds of sensibility, in places receiving the spinal nerves, which are the continuation of the altered roots. Voluntary movements still possible, in bed, and while the patient looks at his limbs, but walking and standing almost impossible. Reflex action *completely* lost in all the anæsthetic parts. If the alterations are in the upper parts of the spinal cord, the other parts being healthy, then voluntary movements in the lower limbs, and even walking or standing, are possible, and may be easy, and these limbs have an increased sensibility and increased reflex actions.

“6th. *Alteration of the posterior columns and of the gray matter in all their length.*—There is no difference between this case and the preceding, except that here there is a real paralysis of voluntary movements, which is complete if the alteration extends to the anterior gray cornua. Greater frequency of formication and of other sensations referred to the periphery.

"7th. *Alteration of the posterior columns and gray matter in any limited part of the spinal cord.*—Very nearly complete loss of sensibility. Degrees of paralysis of voluntary movements varying with the place occupied by the alteration in the length of the spinal cord. Reflex actions increased in parts receiving their nerves from the portions of the cord below the seat of the alteration.

"8th. *Alteration limited to the gray matter.*—The same symptoms as in the preceding cases, except that at first there is a greater degree of anæsthesia than of paralysis, if the alteration begins in the very centre of the cord. Formication and other sensations referred to the periphery, in cases of inflammation.

"9th. *Alteration of the anterior columns in the upper part of the cervical region.*—No paralysis, no anæsthesia, very slight hyperæsthesia, various sensations (particular pain) referred to several parts of the body.

"10th. *Alteration of the lateral columns in the upper part of the cervical region.*—Paralysis of voluntary movements in the four limbs and the trunk. Increased sensibility and greatly increased reflex actions in the paralyzed parts.

"11th. *Alteration of the anterior columns in any part of their length, except the neighbourhood of the medulla oblongata.*—More or less complete paralysis of voluntary movements in all the parts receiving their nerves from or below the parts of the cord where the alteration exists. Slight hyperæsthesia. Reflex actions very much diminished in the parts which receive their nerves from the altered portion of the cord, and increased below these parts.

"12th. *Alteration of the lateral columns in any part of their length, except the neighbourhood of the medulla oblongata.*—Incomplete paralysis of movements. Hyperæsthesia. Diminution of reflex actions less than in the preceding case.

"13th. *Alteration of the anterior half of the spinal cord, including the anterior columns, a good part of the gray matter, and a part of the lateral columns.*—Voluntary movements completely paralysed. Sensibility very much diminished. For reflex actions, as in 11th.

"14th. *Alteration of the various parts of the spinal cord, except the posterior columns.*—Loss of voluntary movements and of all kinds of sensibility. Reflex actions increased or diminished in certain parts of the body, according to the place of the alteration in the length of the spinal cord.

"I know many cases in which one of the two last kinds of alteration has existed. In another lecture, I will relate three of them, which have been recorded by Mr. Cæsar Hawkins, by Dr. John W. Ogle, and by Dr. T. Inman."

The remaining lectures are "On the sympathetic Nerve;" "On the Influence of the Nervous System on Secretion and Nutrition;" "On Epilepsy;" "On the Medulla Oblongata the Pons varolii, and some parts of the Spinal Cord in their relations with the Respiratory Movements," &c. We cannot

afford room to enter on these subjects, especially as we have very recently discussed them all fully in several reviews. We must, however, before closing, explain the hyperæsthesia so constantly found to occur after division or destruction of the posterior or lateral columns of the spinal cord. The author traces this to paralysis of the vaso-motor nerves. These nerves he believes to arise from the spinal cord, and to be distributed to the vessels on the same side of the body as the portions of the cord they arise from. Division of these portions of the cord paralyzes the vaso-motor nerves arising from them, consequently the muscular coats of the small arteries of the same side are also paralysed, the vessels become dilated and there is an increased afflux of blood to the part. As the sensitive nerves of this side cross to the opposite side of the spinal cord on entering it, they are not paralysed by the injury done to the cord, and in consequence of their peripheral extremities having an increased supply of blood a state of hyperæsthesia is induced. Thus we have as a result of division of a lateral half of the spinal cord, loss of voluntary motion on the same side, hyperæsthesia of that side, and loss of sensation with retention of voluntary motion in the limbs of the opposite side.

In taking leave of the subject, we cannot but regret that our author has not adopted fully the theory of the action of the spinal cord propounded by Dr. Todd in that most complete and admirable article of his on the physiology of the nervous system in the "Cyclopædia of Anatomy and Physiology."

We have recently shown in our review of Van der Kolk that the result of all our best microscopical examinations is to confirm and illustrate Todd's theory. We now say the same of Brown-Séquard's experiments and researches, which, while they fully confirm, add but little to what Todd has done; we might almost say nothing, except the fact of the decussation of the conductors of sensation on their entrance into the cord. Some points that are obscure in Séquard's cases might be cleared by adopting the theory that all the spinal nerves terminate in the grey substance of the spinal cord, and that the white columns, instead of being continuations of the nerves through the cord to the brain are commissural fibres connecting the segments of the cerebro-spinal centres with one another. It will be further observed that, though Brown-Séquard devotes much of his lectures to showing what the posterior columns of the cord do *not* do, he nowhere attempts to show, with any precision, what their true nature is, or what functions they *do* perform. Dr. Todd's views on this point are so explicit, and so fully ac-

cord with both the experiments and the clinical observations of Brown-Séguar, that we think it advisable, in order to complete all that we have said, to quote his remarks at length:—

“I have long been strongly impressed with the opinion that the office of the posterior columns of the spinal cord is very different from any yet assigned to them. They may be in part commissural between the several segments of the cord, serving to unite them and harmonize them in their various actions, and in part subservient to the function of the cerebellum in regulating and co-ordinating the movements necessary for perfect locomotion.

“This view is suggested by a comparison of the spinal cord with the brain, and by the anatomical connexions of the posterior columns.

“The brain is an organ composed of various segments, which are connected with each other by longitudinal commissures. The cord is obviously divisible into a number of ganglia, each forming a centre of innervation to its proper segment of the body. These portions must be connected by similar longitudinal commissures to those which confessedly exist in the brain. If we admit such fibres to be necessary to ensure harmony of action between the several segments of the encephalon, there are as good grounds for supposing their existence in the cord as special connecting fibres between its various ganglia to secure consentaneousness of action between them.

“The attribute of locomotive power rests upon the connexion of the posterior columns with the cerebellum, and the probable influence of that organ over the function of locomotion and the maintenance of the various attitudes and postures. If the cerebellum be the regulator of these locomotive actions, it seems reasonable to suppose that these columns, which are so largely connected with it, each forming a large proportion of the fibrous matter of each crus cerebelli, should enjoy a similar function, and that, as they are the principal medium through which the cerebellum is brought into connexion with the cord, it must be through their constituent fibres that the cerebellum exerts its influence on the centre of innervation to the lower extremities and other parts concerned in the locomotive function, and on the nerves distributed to these parts.

“The nearly uniform size of the posterior columns in the different regions of the cord, whilst it may be noted as unfavourable to their being viewed as channels of sensation, may be adduced as a good argument in favour of their being concerned in locomotion and acting as commissural fibres. It is a fact worthy of notice that these columns experience no marked diminution in size until the large sacral nerves, which furnish the principal nerves of the lower extremities, begin to come off. The reason of this is probably because the fibres of these columns connect themselves in great part with the lumbar swelling of the cord, and some of them, perhaps, pass into the sacral nerves.

"The following remarks will serve to explain the manner in which the posterior columns may contribute to the exercise of the locomotive function. In examining a transverse section of the cord in the lumbar region, we observe a great predominance of its central gray matter; the posterior columns appear large, and the antero-lateral columns seem inadequate in proportion to the large roots of nerves which emerge from it. Now, an analysis of the locomotive actions shows, with great probability, that they are partly of a voluntary character, and partly dependent on the influence of physical impressions upon that segment of the cord from which the nerves of the lower extremities are derived. There are two objects to be attained in progression, namely, to support the centre of gravity of the body, and to propel it onward. The former object is attained by physical nervous actions, the latter by mental. The support of the centre of gravity of the body requires that the muscles of the lower extremities, the pillars of support to the trunk, should be well contracted in a degree proportioned to the weight they have to sustain. The contraction of these muscles seems well provided for in an arrangement for the development of nervous power by a stimulus propagated to the centre, and then reflected upon the motor nerves of these muscles. The stimulus is afforded by the application of the soles of the feet to the ground; it is, therefore, proportionate to the weight which presses them downwards. It is well known that reflex actions are more developed in the lower than in the upper extremities, and the surface of the sole of the foot is well adapted for the reception of sensitive impressions. No object can be assigned for this peculiarity, unless it have reference to the locomotive actions, and the great development of the vesicular nervous matter in these regions betokens the frequent and energetic evolution of the nervous force. All the structural arrangements necessary for this purpose are found in the antero-lateral columns. The posterior columns come into exercise in balancing the trunk and in harmonizing its movements with those of the lower extremities.

"Some support is obtained for this view of the function of the posterior columns from the phenomena of disease. In many cases, in which the principal symptom has been a gradually increasing difficulty of walking, the posterior columns have been the seat of disease. Two kinds of paralysis of motion may be noticed in the lower extremities, the one consisting simply in the impairment or loss of the voluntary motion, the other distinguished by a diminution or total loss of the power of co-ordinating movements. In the latter form, while considerable voluntary power remains, the patient finds great difficulty in walking, and his gait is so tottering and uncertain that his centre of gravity is easily displaced. These cases are generally of the most chronic kind, and many of them go on from day to day without any increase of the disease or improvement of their condition. In two examples of this

variety of paralysis I ventured to predict disease of the posterior columns, the diagnosis being founded upon the views of their functions which I now advocate; and this was found to exist on a post-mortem inspection; and in looking through the accounts of recorded cases in which the posterior columns were the seat of lesion, all seem to have commenced by evincing more or less disturbance of the locomotive powers, sensation being affected only when the morbid change of structure extended to and more or less involved the posterior roots of the spinal nerves."—*Cyclopædia of Anatomy and Physiology*, vol. iii. p. 721q.

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*On Diseases Peculiar to Women, including Displacements of the Uterus.* By HUGH L. HODGE, M. D., Professor of Obstetrics and Diseases of Women and Children, in the University of Pennsylvania. With original illustrations. Philadelphia: Blanchard & Lea. 1860. Large 8vo, pp. 469.

THE city of Philadelphia, it appears, is possessed of three colleges, viz., the University of Philadelphia, the Jefferson Medical College, and the Pennsylvanian College. The late eminent Professor of Obstetrics, of the Jefferson Medical College, during his long and brilliant career, shed considerable lustre over the medical school, not only of Philadelphia, but of America at large; and no school of medicine was better acquainted with or more fully appreciated his writings, than that of this country. The late Dr. Meigs was truly no ordinary physician; remarkable not only for his learning, his great practical experience, and the lucid manner in which he expressed his ideas; but also for the high tone of morality, both professional and general, which pervaded his life and actions. None could have read his works without coming to the conclusion, that in him was concentrated every quality to render him the beau idéal of the experienced physician, the scholar, and the Christian gentleman.

The University of Philadelphia is at present as highly favoured, as the Jefferson College was some little while ago, in the possession of a professor of obstetrics, by no means unworthy to be ranked as the brother and friend of the late Dr. Meigs. Dr. Hugh L. Hodge was the college companion of Meigs, and his fellow-labourer in the great city of Philadelphia, where they had, up to the year just passed, "toiled together for upwards of forty years in the arduous and responsible duties of their profession." It does not astonish us to hear from Dr. Hodge, that "the chain of their friendship had never been

broken, or even sullied;" for, though we never had personal acquaintance with Meigs, yet we can well conceive, how difficult it would be to break a friendship with such a man, even for a single moment. It is no unworthy introduction, this, of the author's, for a man whom Meigs could take into his confidence and urge "to make a sign to posterity," must have evinced to him the capability of doing so and the worthiness to undertake the task.

The result of Dr. Meig's persuasions has been the volume before us, which, on perusal, we have found to be unlike the generality of books of this nature now-a-days met with. It is not a compilation, or book of excerpts, clipped from the works of others, but one consisting of "the results of his own observations and reflections towards the improvement of medicine." Such a book is at least refreshing; but Dr. Hodge's shall be found useful and instructive, as well. The volume is divided into three parts; the first consisting of "diseases of irritation;" the second of displacements of the uterus; and the third, of diseases of sedation. By the latter term is meant inertia. One great object of his work "is to maintain that in many cases the independence of nervous disease is complete; and also that when complications exist, demanding therapeutical assistance, the neurotic affection is often of primary and essential importance, and demands the chief attention of the practitioner." Making nervous irritation the main-spring, he goes on the old axiom, "*ubi irritatio ibi affluxus*," and thus accounts for the subsequent occurrence of congestions, inflammations, &c. But that the reader may have a better idea of his views, we give the following passage in the author's own words:—

"The hysterical, or the neurotic diseases of women, are therefore states of irritation of the cerebro-spinal nervous system in whole or in part. They are very evanescent when the cause is transitory, as in nervous affections from moral causes, such as fear, anger, anxiety, joy, grief; or from physical causes, as from indigestion, flatulence, &c.: but very persistent in all cases where the cause remains operative, and will then often defy the best directed remedial agents for months and years. Hence the indomitable character of various nervous or neuralgic diseases; the cause is persistent, perhaps it has not been detected, or cannot be removed. Tonics, nutritious diet, exercise, travelling, as well as antispasmodics, narcotics, and stimuli, prove useless, or perhaps worse than useless, and at best are but temporary palliations. The symptoms will return, often with increased vehemence. The location of the cause may be in any tissue or organ of the body. Wherever the irritability or sensibility of a part is disturbed, thence, as from

a focus, may radiate nervous excitements. They are usually perceived in the nerves involved, first at the point irritated; then, if sufficiently severe, in the sentient extremities of such nerves, or in the reverse direction towards their origin in the spinal marrow or the brain, and thence again by reflex influences in any tissues of the economy; thus causing a disturbance of the functions of such tissues or organs, so far as these functions are dependent on nervous influences. In other words, 'nervous irritations' of the brain and spinal axis, and of their dependencies, may, and often do, arise from any local irritation. It is often of small consequence what may be the character of such local irritation; whether it be simply nervous or inflammatory, sympathetic nervous irritations may result in different and often in very distant portions of the animal economy.

"Odontalgia and otalgia often excite neuralgic pains and spasms of the nerves and muscles of the face and neck, and sometimes severe headaches, and other cerebral disturbances. Often croup, asthma, and palpitations of the heart, as well as gastrodynia and colics, are excited by indigestion; and by the same cause headaches, delirium, and convulsions, are very frequently induced. Hysteric convulsions and epileptic spasms, as well as the more dangerous tetanic affections, are constantly excited by the presence of a thorn in the flesh, by a local painful inflammation, or other organic irritations. The proof seems positive to the medical philosopher, not merely that such nervous disturbances follow local irritations, but that they disappear almost instantaneously on the removal of such affections, while they remain obstinate under the best general treatment if the local irritation be allowed to continue. The continuity of tissue, and the wonderful unity of the nervous system, together with the inconceivable rapidity by which impressions are transmitted in health from the circumference to the centres, and from the centres to the circumference, afford a most satisfactory explanation of the transmission of such morbid irritations, and of their reflex influence to distant parts, or to the whole of the cerebro-spinal apparatus.

"A complete history of nervous diseases would involve, therefore, an account, if it were possible, of the innumerable causes, whether intellectual, moral, or physical, which may disturb the sentient system. The nervous and cerebral symptoms being, after all, not strictly the disease, but the *indices*—as they are the results of the primary impressions of the local irritation. Such a task would be endless, and in a great degree fruitless. My object is far more humble and limited. It is to illustrate the general truths already enunciated, by a detailed account of those nervous disturbances which are excited, or kept up by the uterus and its appendages when in a state of morbid irritation, and hence to deduce the proper pathology and therapeutics of irritable diseases in women.



"The general principles applicable to all the variety of neurotic complaints will, it is trusted, be thus evolved and elucidated, inasmuch as no organ of the body has so constant and direct an intercommunion with the nervous system, as the uterus; and inasmuch, also, as the peculiar physiological and pathological character of the female sex is most intimately and indissolubly connected with this organ and its appendages."

So far, then, as we can follow the author in his reasonings—and to us, in some passages, he is rather obscure; we conceive he makes one prime or original starting point for uterine disease—viz., "irritable uterus," which diseased condition has reference, not to its circulating system, and, of course, not to its organic life, but to its nervous system, its animal life. "It has passed from a healthy to an unhealthy state. As regards its nervous system, it is morbidly sensitive; it is irritable." The organic actions are not affected, and there is no alteration of structure, even after the lapse of years. This, then, is, doubtless, a very good definition of irritable uterus, and the disease itself is admirably described; but all other affections of the uterus, which we have been heretofore in the habit of looking on as distinct diseases, our author includes under the head of complications of irritable uterus, except such as arise from mechanical irritation, or the like. Irritable uterus is the chief disease, then we have inflammation in all its forms, tumours, cerebro-spinal irritation, reflex influences, and menstrual disturbances, all, as complications of that primary state of the organ.

Although the author is, to our mind, a little confused in laying down his reasonings, yet there is much ingenuity apparent in his endeavours to prove the soundness of his views. Irritable uterus is certainly his hobby, and he uses it, perhaps a little too hard. It must be remembered, however, as above alluded to, that he considers inflammation is sometimes the original disease, and then irritable uterus is secondary or consequential. Still irritable uterus must be brought into the case, whether primarily or secondarily.

In dealing with what he calls the complications of irritable uterus there is much matter to be commended. For example, he says, "in chronic cases of endometritis"—a term by which he designates inflammation of the uterine mucous membrane—"the mucous tissue,—

"particularly at the lips of the os uteri, loses its natural smooth character, becomes slightly prominent, and rough as well as red; precisely as in similar chronic inflammations of the mucous covering

of the eyeball, 'the tunica conjunctiva;' there by the surgeons termed 'granular conjunctivitis,' or a 'granular condition' of the tissue, from its great similarity to the appearance of the granulations of a simple healing ulcer. This 'granular inflammation' of the neck of the uterus, according to my experience, is not very frequent; but by others has been reported as exceedingly common. It has been described and exhibited as 'ulcers' of the uterus, and, considered either by itself or in connexion with its cause, the inflammation of the membrane, as the real source of mischief and as giving origin to all those symptoms which have been in this work attributed to 'irritable uterus.' This, however, is not proper ulceration. There is an exfoliation of the epithelial scales, a development of the papillæ of the basement membrane with more or less turgescence and elevation of the inflamed surface; but there is no 'solution of continuity' in the tissue, no depression, no loss of substance, no generation of new material—as in a granulating ulcer. And, when recoveries ensue, there is no cicatrix, no thickening, no contraction of the tissues, and no consequent alteration in the form of the part affected. At the os uteri, the mucous membranes and the parts adjacent, precisely as in analogous states of the conjunctiva of the eye, return to their original normal state, and the tissues are as perfect as if they had never been inflamed. I have never seen an ulcer of the uterus a proper 'solution of continuity' of the basement membrane, *except* as the result of contusions, or wounds, or of peculiar, specific, or malignant disease. All this may be regarded, perhaps justly so, as a debate as to the meaning of the word 'ulcer.' But 'words are things;' at least they ought to represent things and facts correctly. The Hunterian definition of an ulcerated surface (as intimating a solution of continuity, a destruction of tissue) has been so universally and so long received by the profession, that to extend its employment to a condition of the tissues where there has been no destruction of tissue, is to confuse the minds of practitioners as well as of students, to convey incorrect ideas of the views of the writer, erroneous pathological notions, and to cause manifest and mischievous errors in practice. All these evils, and much greater, in my judgment, have resulted from the application of the word ulcer to such inflamed surfaces of the uterus. The bad influence on the mind and imagination of suffering nervous women is by no means the least of these evil consequences. Ulcers with them are associated with the idea of destruction, of an 'eating' process, of something dangerous, intractable, malignant. And there have not been wanting those in and out of the profession who have magnified these dangers and stimulated the excited mental sensibilities of the patient for their own selfish, sordid purposes. Empiricism and ignorance have gained a rich harvest from this apparently trifling source. Often, very often, have I examined patients, who were reported by physicians and others to have ulcers where none could be detected, or to have had them where no vestige of them, no hardening, no irritation, would possibly be found."

We regret to say that, even in this country, the speculum has been abused; and we agree with Dr. Lee, of London, that nearly everything of importance can be decided without it. It should be remembered, as the author endeavours to impress on his readers, how small a portion of the uterus can be seen, and how it is virtually a viscus surrounded by other viscera, and all but removed from the possibility of ocular investigation. And we would add, when the little that can be seen is brought into view, how very seldom is anything of importance (the commencement of malignant diseases excepted) observed, except what has been wrongly termed "ulcer of the womb," the mere abrasions or granulations above alluded to, the harvest of the unprincipled.

In the section on displacements, the author gives us excellent descriptions of the various uterine dislocations, together with their respective diagnosis and the appropriate treatment for each. Though we have not observed much of novelty in this portion of the volume, still it is characterised by an originality and clearness of description which renders it most readable. Besides, there are several well-executed diagrammatic wood-cuts, illustrating in a most happy manner the anatomy of uterine malpositions, and the mechanical appliances for the treatment of each kind of displacement.

Amongst the various mechanical supports which have been suggested from time to time to combat these displacements, that for the cure of retroversion as invented and employed by Dr. Simpson, of Edinburgh, has not been forgotten. It will be remembered by our readers that this consisted of an external frame-work, a vaginal stem and pessary, and a style to project into the cavity of the neck and body of the uterus, and that this instrument, when adjusted, was worn for a considerable period. It will also be in the recollection of many, that the use of this instrument was severely commented on by Dr. Lee, of London, some time ago, and was christened by him "the uterine impaling machine". Bearing this in mind, it may be interesting to hear the opinions of the Philadelphia Professor on this instrument. We shall quote him at length. He says—

"Professional attention has been turned to this 'intra-uterine pessary,' as it is termed, by the talented but enthusiastic Professor in the University of Edinburgh, Dr. Simpson, with whose valuable suggestions on many subjects the profession are familiar. His

\* Clinical Reports on Ovarian and Uterine Ovaria. Robert Lee, M. D. London: 1853.

course is, however, to be followed like that of most pioneers, "cum magnâ prudentiâ longo intervallo:" especially in the employment of this instrument, very efficient in restoring or fixing the uterus in its normal position, but fraught with many dangers."

Again:—

"There are, we think, so many serious objections—theoretical and practical—to be urged against this apparatus, that it would seem to be impossible that it should come into general use. Its employment will probably be confined to special cases, or restricted to those few individuals who are so exact in their diagnosis as to estimate aright the peculiar cases to which it is adapted, and so cautious in their practice, as to watch their patient day after day, not to say hour after hour.

"One important objection to the instrument, as proposed, is the oxidizable character of the metals employed—German-silver, or a mixture of silver and nickel. Dr. Simpson has also employed lead, and copper, for the vaginal disk. These metals, from the moisture, atmospheric air and heat in the vagina, will oxidize with more or less rapidly, and thus be corroded; and their oxides, combining with the acid secretions of the tissues, may become more or less irritating. The effect, if any, in preserving the metals in their natural state which may arise from any galvanic influence due to the combination of different metals, must be decided by the chemist and by observation.

"The second objection is from the frequent necessity of employing napkins, tapes, and bandages, with all their uncleanness and frictions, especially in warm weather, with the absolute necessity of repeatedly changing them, as have been specified when speaking of external supporters.

"The external frame-work itself is a great objection, rising up in front of the sensitive tissues of the nymphæ, clitoris, and labia, and liable to produce more or less friction and pressure; its mobility is, with all care, often so great, that it cannot be worn without additional applications, as tapes or bandages.

"The vaginal stem must excite more or less irritation against the urethra, nymphæ, and other sensitive tissues, which few patients will bear.

"The fixed character which all vaginal stems give to pessaries has already been pointed out as a most serious objection. They never yield to pressure from above. The internal irritable tissues are pressed against them forcibly at all times when the patient is erect, and often violently in coughing, vomiting, sneezing, &c. In the case of an intra-uterine pessary, this objection is stronger than in all others, as the points of internal resistance are, first, the mouth of the uterus; secondly, its internal surface; and thirdly, in some cases, the upper or transverse side of the triangular cavity of the uterus, directly against the point of the intra-uterine stem.

“The chief pressure is received against the orifice of the uterus, and the lower extremity of the cervix, which rests on the vaginal disk—a fixed point. In all the impulses from above, the impression is made on these delicate and irritable tissues, so that sometimes the orifice is even dilated by the pressure against the disk. It has already been pointed out in detail, that the results of such pressure in neglected cases of prolapsus uteri, against even the soft parietes of the vagina, are flexion, soreness, nervous disturbance, pain, inflammation, continuing for years, with the granular, and sometimes, it is said, even with the ulcerated condition of the mucous membrane. It was also shown that such inflammations of the os were aggravated by the ball, the concave disk, the double and plano-convex pessaries, even though they were movable in the vagina. The theoretical conclusion, therefore, is clear, that more irritation would ensue when the disk is held immovable, as in the intra-uterine pessary, against the orifice by means of the stem.

“I experimented several times with the intra-uterine stem and disk, generally in women whose catamenia had ceased, and where no marked sensitiveness existed in the uterus. In each, after a few days, leucorrhœal and bloody discharges ensued, with considerable pain and irritation, sufficient to indicate the propriety of discontinuing the instrument.

“The intra-uterine probe is said to be worn with impunity. The testimony given will never be acted upon without great caution, if we reflect upon the sensibility of the internal tissues of the uterus, so great indeed that a uterine sound can seldom be introduced, especially through the internal os uteri, without causing severe pain, sometimes agonising, if we may judge from the exclamations of the patient, and from the intolerance of the uterine cavity of fluids, simple as well as stimulating. The vascularity and delicacy of the lining membrane are so great, that probing the uterus is generally followed by the effusion of blood; and the slightest mental, as well as physical excitement, will often produce leucorrhœal and menorrhagic discharges.

“In the case of the intra-uterine pessary, the internal stem is not merely always in contact with these sensitive vascular tissues, but presses upon them more or less firmly during every motion of the body, as this stem constitutes the means by which the organ is kept in position. It is not, therefore, to be wondered at that in irritable and congestive, as well as inflammatory, states of the uterus, this instrument, as is acknowledged, cannot be borne, or that it has sometimes to be removed because leucorrhœal or menorrhagic discharges are excited or increased. The greater wonder is that such irritations and discharges are not the constant and unavoidable result in all cases. That such is not the uniform effect can only be explained by that law of vital tissues by which, under constant pressure, if not too severe, parts become gradually more insensible to irritations.

"The most serious pressure is, however, to be apprehended from the point of the instrument against the internal upper extremity of the cavity. Dr. Simpson has attempted to obviate this danger by making the instrument shorter than the long axis of the cavity. The internal stem measures two inches and one-third, while two inches and a half is the usual length of the uterine axis internally. The difference is merely two lines, or one-sixth of an inch! What possible security can be given that, in the various motions, this small space of one-sixth of an inch will not be obliterated, and that the fundus will not impinge against the point of the probe? If the uterus were perfectly firm—as if made of metal, of bone, or even of cartilage—such an accident could not perhaps occur; but the tissue of the uterus, although firm, is flexible, and bending of the organ is a fact of daily observation. With an intra-uterine supporter there can be, it is true, no marked lateral, anterior, or posterior flexion, the metallic rod within resists such changes; but the whole superincumbent pressure must force the fundus downwards, and as the opposite extremity of the organ is fixed by the vaginal disk, the yielding of the uterus will, therefore, be from above downwards, in the direction of its length: all such yielding will endanger the forcible impinging of the uterus against the point of the internal stem. This danger is aggravated by the gradual dilatation of the os, and the eversion of its lips, so that the stile penetrates still deeper into the organ. Either of these circumstances—and, of course, still more when both are in unison—can readily shorten the perpendicular diameter of the uterus one-sixth of an inch, when mischief of a serious character would ensue—inflammation, ulceration, even, it may be, perforation of the uterus, and peritonitis with its dire consequences. Such results are said to have occurred, and certainly are to be apprehended, especially as few are expert enough to measure and arrange the safe proportions which ought to exist between the length of the stile and that of any individual uterus. Dr. Simpson speaks of using one some three and a half inches long in a hypertrophied organ with impunity; but one of our western physicians, less skilled in diagnosis, inserted an intra-uterine pessary, with a stem five inches in length, into a uterus whose length was found to be but three inches when measured by the uterine sound. The instrument is in my possession.

"This particular danger can easily be lessened in a great degree by diminishing the length of the stem, so as to allow half an inch or six lines between the internal surface of the fundus and the point of the stem. Nevertheless, the strong objections of the uterus being firmly fixed by an immoveable pessary, and the irritation arising from the intra-uterine stem, &c., must be regarded as of serious import. Minor objections may be alluded to, such as the occupation of the vulva and vagina so as to prevent the natural uses of this canal; and the irritations and discharges which the vaginal stem would usually produce."

We are certain that such an instrument could never come into general use in this country, and we are glad that the author has given such a guarded, in fact, we may say unfavourable, opinion, of it. But we cannot agree with him, that "further reflections and experience may ameliorate the objections;" he has detailed, or "may suggest modifications in the instrument which may render it more safe, and not impair its efficiency." Under all circumstances, we object to the introduction of metallic implements into the cavity of the uterus, and more especially to their maintenance there for an uncertain and, in some cases, an unlimited period. So that we do not expect much benefit to arise, even from the author's modifications of Simpson's "impaler;" nevertheless, we feel it to be our duty to give insertion here to his description of it:—

"*Simpson's Intra-Uterine Pessary modified.*—I venture to present a drawing of the modification, just alluded to, of Dr. Simpson's intra-uterine pessary. As will be observed, a vaginal elliptical ring was substituted for the external frame-work, and the intra-uterine stem made shorter. The experiment, as regarded the maintenance of the uterus in position, was a perfect success, and at the same time more mobility was allowed to the organ under the muscular efforts of the patient, and all the irritation and annoyance due to an external apparatus were done away with.

"The intra-uterine stem was made but two inches in length for a uterus measuring two and a half inches internally. The vaginal disk had attached to a point in its circumference a short stile four lines in length, with a small foramen at its outer extremity. The disk with its stile should make, with the intra-uterine stem, an acute angle, about  $40^{\circ}$  to  $50^{\circ}$ , corresponding somewhat to that made by the axis of the uterus with that of the vagina. An elliptical ring was then made, slightly curved in its length, and two inches and a half long by two inches broad (but of course the size must vary with the size of the vagina), and with a perforation in a longitudinal direction, through the bar at one extremity to receive the stile.

"Before introduction, a strong thread or a fine flexible wire is to be passed through the eye in the vaginal stem. The pessary is then passed into the vagina, the stem carefully introduced into the cavity of the uterus, and the organ may be partially elevated. The string, hanging out of the vagina, should now be carried through the opening in the extremity of the ring. The ring is then introduced into the vagina, with its convexity toward the bladder, and the string being made somewhat tense, acts as a director, by which the ring can be pushed so as to cause the vaginal stem to enter the foramen in the ring. The string is now to be withdrawn; the pressure of the vagina, &c., will keep the ring firmly fixed on the stile, so that the direction of the intra-uterine probe represents

that of the axis of the uterus, and the pessary that of the vagina. If the uterus be not perfectly restored, a little pressure on the ring will accomplish this purpose; and the anterior wall of the vagina under the influence of the superincumbent weight of the viscera, increased by muscular effort, will constantly depress the pubic extremity of the ring towards the perineum, and thus compel the uterus under the command of the stem to follow its motions. All these tissues being pliable and elastic will allow a certain degree of yielding of the uterus to the distention of the bladder, bowels, &c., and thus the instrument does not materially interfere with the mobility of the womb.

"The removal of this instrument is very readily accomplished by drawing the elliptical ring towards the orifice of the vagina, which detaches it from the stile, after which the vaginal disk, with the intra-uterine stem, can readily be removed. If by any arrangement of this kind too much lateral motion should be allowed to the uterus from the cylindrical form of the stile in the vaginal disk, this can readily be prevented by making it and its sheath in the extremity of the ring, flat, so as to prevent rotation.

"Whatever modifications, however, may be suggested, by which the evils are moderated, yet the intra-uterine pessary ought to be regarded with great suspicion, and be very cautiously employed, even in the very limited number of cases to which it is at all applicable."

We fully concur with the author's expressions conveyed in the last sentence.

The volume concludes with a few pages on what the author terms "diseases of sedation," which term is tantamount to "inertia," an older and much better word. But "sedation," we presume, has the charm of being somewhat allied to the word "location" (one frequently made use of throughout the work), so that we shall take "sedation" to be "inertia" Americanized. The chief feature of this section is, of course, amenorrhœa and its concomitants; and though short, yet it is a good dissertation on the disease, containing an excellent *resumé* of all that is known theoretically and practically on the subject.

The great worth of Dr. Hodge's book is, that it conveys the experience of one long engaged in the practice of obstetric medicine; and though as a work it is decidedly inferior to that which emanated from his revered colleague Meigs, still it is in every way worthy of the school of Philadelphia, long since rendered famous by the eminent Professor of the Jefferson College.



*Diagrams of the Nerves of the Human Body.* By W. H. FLOWER, F. R. C. S. E. London: Churchill, 1861. Folio, pp. 8. With 6 Plates.

THIS is a very timely publication, and one of equal use to both the student and the practitioner. The difficulty of acquiring a thorough knowledge of the nervous system is only equalled by the difficulty of retaining the knowledge when it has been acquired, as our readers must themselves but too well know; and therefore we are certain they will gladly avail themselves of the assistance to the memory which Mr. Flower's beautifully executed plates so fully affords. In five large folio plates—

“the distribution of all the nerves of the body, so far as the branches have received distinctive appellations, is shown; and their divisions are traced to the muscles and to the various regions of the cutaneous surface. To afford greater facility for reference, the names of the muscles are printed in red letters, those of the nerves being black. It must be clearly understood that the plates are only diagrams, or plans; and that in reducing to a plane surface objects which are in reality superimposed at various distances, and which sometimes cross one another, their mutual relations and proportions must often be disarranged.”

The eye, however, after a little study, becomes accustomed to this necessary mechanical arrangement; and to all who have acquired their knowledge of the anatomy of the nervous system by dissection, is productive of little inconvenience. It would be far different, however, were it attempted to teach the student in the first instance from these plates; such a mode of study could but lead to erroneous ideas of the nervous distribution, and to confused notions. We therefore recommend the use of these plates to advanced students only, and to the practising physician and surgeon; and to these we warmly recommend their employment both for reference and for study. As might be expected from the position the author holds as Demonstrator of Anatomy at the Middlesex Hospital, they are carefully and correctly executed; and we cannot bestow too much praise on the lithographer, the printer, and the publisher, for the manner in which Mr. Flower's drawings have been presented to the profession, and his ideas carried out.

*Ten Lectures, Introductory to the Study of Fever.* By ANDREW ANDERSON, M. D., Lecturer on the Practice of Medicine in Anderson's University, Glasgow. London: Churchill. 1861. 8vo, pp. 180.

THERE is nothing more difficult for either an author or a reviewer than to estimate justly what is the precise amount of elementary character to infuse into a work which is professedly only an introduction to the study of the subject-matter of which it treats. The work before us illustrates the difficulty in question in a remarkable degree. It is evidently the production of a man of learning and practical experience. But we are more than doubtful as to its utility in the hands of persons not already pretty well versed in fever pathology. Dr. Anderson's lectures will be read with interest and profit by men who have seen and treated fevers. How far they are so framed as to supply to the student the *prolegomena* necessary for entering on the consideration of special fevers subsequently, is what we are somewhat sceptical about.

How, for example, can it be supposed that the brief and cursory notices of the general pathology of fever so lightly and passingly sketched in the first chapter, furnish sufficient of even the most elementary knowledge on the profound questions involved. And yet it is impossible to glance through this section without a conviction that the writer is well versed in what he so hastily skims over; and a regret rises to the mind that an author who promises so well has not achieved more. On the classification of fevers Dr. Anderson seems to share the uncertainty of most of the fever pathologists of the day; and his arrangement and grouping of the several varieties of fever are evidently to be accepted as only provisional. But besides a classified view of the several distinct fevers as such, the author, "speaking of fever irrespective of any particular poison," recognises a division of fever in general into eight different **TYPES**—"in other words, eight distinct phases which fever may assume." Without more precise definition than the author has furnished, we fear that a category of types and a category of classes can hardly fail to confuse the student. What it is intended to convey is, evidently, that *any* fever of *any* class may present this or that type of febrile action, according to the condition of the constitution for the time being, and the causes which have called the fever into play. Thus a case of scarlatina, measles, or small-pox, may present a "mild," or "toxic," an "inflammatory" or an "asthenic"

type. The author recognises eight different types, viz., the "mild," the "toxic," the "congestive," the "inflammatory," the "asthenic," the "nervous," the "septic," and the "typhus."

These several types are discussed at some length in the earlier portion of the second chapter; but here again we have to complain of the author's inadequate treatment of an important subject which he manifestly understands well. The same must be said of the pages devoted to the "complications," "sequence," and "management" of fever, in all which sound doctrine, based on practical knowledge, is fully evidenced; but the treatment of the subjects is too rapid and generalising for the uninitiated, and for the most part, simply unnecessary for those who have themselves investigated the pathology of fever.

The classification of fevers adopted by the author would appear, as we have before stated, to be of but a provisional nature. It is as follows:—

I.—*Simple Fevers:*

1. Ordinary ephemera and synocha.

II.—*The Typical Eruptive Fevers:*

2. Chicken-pox.
3. Small-pox (Cow-pox).
4. Scarlet fever.
5. Measles.

III.—*The Imperfect Exanthemata, or ordinary continued fevers of this country:*

6. Typhus.
7. Enteric fever.
8. Gastric fever.

IV. *The Bilious Fevers:*

9. Relapsing fever.
10. Malarious fever.
  1. Intermittent fever.
  2. Remittent.
11. Pestilential fever.

V. *The Phlegmonous Fevers:*

12. The plague.
13. Puerperal fever.
14. Erysipelas.

VI. *The Adynamic Mucous Fevers:*

15. Diphtheria.
16. Influenza.

The arrangement is one open to objection in many points of view; but it has long been our opinion that in the present position of nosology, any simple categorical exposition of the fevers, practically recognised as distinct, is to be accepted as such on the responsibility of the author who employs it. A strictly scientific classification of fevers seems for the moment unattainable.

Not a few will be disposed to quarrel with Dr. Anderson for his *prononcée* opinions on the subject of contagion. His views on this subject break forth in several places; but in none more strongly than when, speaking of the exanthematous fevers, he says:—"All these fevers are contagious—all of them *very* contagious, especially towards convalescence; and there is no evidence that they ever arise from any other source than this—each from a previous case of the same disease." It may be asked, had Cain or Abel small-pox? and if so, following the Linnæan theory, are we to assume that this, and similar entities of diseases, begotten like from like, have been transmitted in unbroken parallel lines, from prototype diseases engendered amongst the first children of Adam? Or, on the other hand, are we to seek for the development of the numerous species of diseases from a common morbid state, from which all those diseases now existing have branched in divergent lines?

In speaking of small-pox, the author cites from the private letter of a friend the following remarkable and interesting passages, corroborative of the belief now entertained by many, that forms of disease identical with those which prevail in the human subject are not by any means so unknown amongst the lower orders of creation as was at one time supposed.

"In the year 1841, I was in the province of Veragua, in New Granada, to the north of the Isthmus of Panama, and left the town of St. Jago, on the western coast of David, in Chiriqui, a town in the interior, about sixty or seventy miles to the N. E. (and leeward) of St. Jago.

"The small-pox was raging with great violence in St. Jago, but there was no appearance of it in David. A few days after my arrival there, taking my customary morning's ride in the forest, which teems with animal life, I was struck by observing one or two sick and apparently dying monkeys on the ground under the trees. The next morning I was struck by the same singular appearance (for it is very unusual to find a wild animal sick; they instinctively hide themselves), and by thinking that I perceived several on the trees moping or moving about in a very languid and sickly manner. I consequently dismounted, and carefully examined two which were

on the ground—one dead, and the other apparently dying; and after careful examination, no doubt remained on my mind that they were suffering and had died from small-pox. They presented every evidence of the disease; the pustules were perfectly formed; and in one instance (that of the dying one), the animal was nearly quite blind from the effects. A few days afterwards (I think about four or five days) the first case of small-pox appeared amongst the inhabitants of David; and in the course of a fortnight, one-half of the population was stricken."

This observation is one of much interest and value. It illustrates the old law so well and distinctly recognised by the master-mind of Homer, that epizootic disease precedes the epidemic invasion of the human race:—"Ουληας μεν πρωτον επωχαιο και κυνας αργους," as the Coan bard narrates of the plague sent by the wrath of Apollo upon the Grecian forces.

Passing over the sections on scarlatina, typhus, and enteric (typhoid) fever, which offer little for remark except their extremely light and cursory character, our attention is arrested by the heading, "Gastric Fever," which the author believes—

"to be a separate disease, on the like ground to that on which I hold enteric fever to be different from typhus. \* \* \* The affection of the stomach distinguishes it from enteric fever, while that of the bowels is quite different from the follicular lesion proper to that disease, but to which there is here no tendency, though there is often very great irritation of the mucous membrane going on even to ulceration; and we have a diarrhœa, but quite unlike that which I have described as occurring in enteric fever. Gastric, like enteric fever, I believe to be contagious, though in a less degree than the proper exanthemata; and, like all other fevers, it may prevail epidemically. More than most it is, I believe, connected with what early in these lectures, we called 'effluvia,' emanations, that is, from decomposing animal matter, which seem to me sufficient of themselves to produce the disease; and it often assumes more or less of a remittent character, which is not observed in the fever we have hitherto been describing. Its *eruption* is the most obscure of the three which belong to the imperfect exanthemata, and also the most inconstant. It does not appear, like that of typhus, over the whole body, but like the spots in certain pores only on the epigastrium and abdomen; yet, unlike them, it is not elevated nor pink. It consists of a few small, insignificant-looking red spots, which you would probably not observe unless you looked for them, and which appear about the seventh day of the disease."

Dr. Anderson describes this gastric fever as "most irregular in its stages, form, degree, and symptoms;" and we submit it

as worthy of consideration, whether it may not be with advantage regarded as an errant type of "enteric fever," rather than as a distinct affection. At all events, we must consider the author's case for this fever as a disease distinct on the one hand from enteric or typhoid, and on the other from the gastric remittents of childhood, as "non-proven." Some brief account of yellow fever, and also of the "mucous fevers," diphtheria, and influenza, concludes the work; but these sections demand no special notice at our hands. With these observations we must close our notice of Dr. Anderson's Lectures. We have perused them with interest, and not without profit; but we could yet wish that he had dealt with such important topics in a more full, comprehensive, and exhaustive manner.

## PART III.

### MEDICAL MISCELLANY.

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#### TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY\*.

(Continued from p. 255.)

SESSION 1860-61.

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JANUARY 23, 1861.

DR. BAINE in the Chair.

*Cases of Lithotomy operated on in the Cork South Infirmary.*—By WILLIAM KEARNS TANNER, M.D., L. & F.R.C.S.I., President of the Society; Senior Surgeon, Cork South Infirmary; Surgeon, Cork Lying-in-Hospital; late Lecturer on Surgery, Cork School of Medicine, &c., &c.

Stone in the bladder must be considered as one of the most serious diseases with which the human body is liable to be afflicted.

When considered either in respect to its dangerous nature, the lamentable suffering it causes, or the great judgment it requires to determine upon and rightly select the best means for its cure, and then to accomplish so important an undertaking successfully, is indeed a serious matter. I now intrude on the valuable time of this Society for the purpose of eliciting opinion, and causing a discussion on the most difficult and interesting points which have occurred during my treatment of those unfortunate sufferers afflicted with this dire malady, and for this purpose beg to relate the details of some of my cases, each presenting different characters and a corresponding difference in their treatment.

The first was that of a little boy, aged five years, from near Belgooly, admitted into the South Infirmary on the 10th of March, 1854. He had been afflicted for nearly two years, during which time he was subjected to different plans of treatment. He was

\* These Reports have been furnished to us by Dr. S. Henry Hobart, Secretary to the Society.—Ed.

much emaciated, and had a pale, haggard look. The rational symptoms of stone were well marked. He suffered intense pain at the orifice of the urethra after micturition, and his prepuce was greatly elongated from constantly pulling it. He had occasionally a sudden stoppage of his urine, which he generally was able to relieve by a change of posture; and the torture which he suffered in the back, loins, and perineum, extending on rough motion down the groins and even to the soles of his feet, was not to be described. When he had a fit of the stone, it was accompanied by frequent micturition; and in consequence of the straining, prolapsus of the anus to a considerable extent existed. His urine was alkaline, and contained mucus, with a small quantity of pus, but no trace of albumen could be detected. From the irritation set up in his bladder, causing slight chronic inflammation of its mucous membrane, I ordered him a hot bath, mild laxatives, demulcent drinks, for the purpose of bringing it to a more healthy state before sounding him, which was accomplished after a few days, when I introduced the metallic sound, and distinctly felt and heard it strike against what appeared to me to be a large, but soft, calculus. On consultation with my colleagues, we unanimously agreed that the operation of lithotomy would be the most likely to succeed, in consequence of the youth of my patient and the great size of the stone (though his bladder was then capable of retaining a considerable quantity of urine, and his power of emptying it unimpaired). We accordingly fixed the day but one after, for the operation, and at that time proceeded to accomplish it, having on the previous evening administered a small dose of castor oil, and in the morning given him an enema for the purpose of emptying his bowels, he was then placed in the usual position for the lateral operation of lithotomy; and being properly secured, and having ascertained that his rectum was empty, I introduced a full-sized and deeply-grooved staff (on its convex surface) into his bladder, and having, without difficulty, felt the stone, I gave the staff into the right hand of my assistant, who directed its groove a little to the left side, but still keeping it tolerably well up against the pubes, whilst with his left hand he drew up the scrotum, and made the perineum tense. At this moment my little patient endeavoured to empty his bladder (which he had been prevented doing for some hours previously), and whilst straining caused a considerable prolapsus of the rectum, which I found it quite impossible to keep reduced, and therefore proceeded with the operation, which I commenced with an ordinary scalpel, by sinking it deeply at the left side of the *raphé* of the perineum, about one inch and a quarter in front of the rectum, and continued my incision, by cutting deeply into the space between the erector penis and accelerator urinæ muscles, and pushing it up, as it were under the pubes, until I struck the staff between the bulb of the urethra and prostate gland, when I gradually drew my knife downwards, outwards, and more towards the surface, until I brought it a little further back than between the tuberosity of the ischium,



and protruded gut, which I protected from the knife, having drawn it aside with my finger; I then felt in the wound with the fore-finger of my right hand for the groove in the staff, which I took from my assistant, and held in my left hand, a course I consider always necessary to take for the purpose of feeling, through the sympathy between both hands, that the groove in the staff is bared in the proper place, and to be a guide in conveying knowledge of the relative anatomy of the parts. I dwell on this point, from the difficulty which I experienced, in one of my earliest operations, in getting into the groove of the staff, but which was quickly and easily overcome by this means. This manipulation occupied but a few seconds; and handing back the staff to my assistant, I guided the knife with the fore-finger of my left hand into the bared groove of the staff, and feeling it securely placed, and depressing the handle, pushed it steadily into the bladder, when the urine flowed; then, having withdrawn the knife with care, I passed my finger easily into the bladder, and ascertained that the wound in the prostate was of quite sufficient size to admit of the easy entrance of the lithotomy forceps; and, what I consider of most importance in this operation, that it did not extend beyond the prostatic portion of the bladder. I then felt the stone, which was enormously large; and my assistant having withdrawn the staff, I introduced the forceps, guided by my finger, with which I then felt the calculus, when, having removed my finger, I easily and firmly grasped it, but the handles of the forceps being very far apart, I feared having caught it in its large diameter, when I let it go, and tried it again, but not with much better success. I then proceeded to use gentle traction, and, as I increased the force, the stone formed a globular perineal tumour, of very considerable size, when I was strongly pressed by some of my friends to enlarge the opening in the left side of the prostate; but I firmly declined to do so, with the approval of my colleague, Dr. Gregg, knowing the danger likely to result from extending the incision beyond the prostatic portion of the bladder; but whilst I was thinking of the total impracticability of removing the stone by the ordinary means, and of what course would be the most judicious for me to take for that purpose, it suddenly crushed in atoms between the blades of the forceps. I then removed some of the larger fragments with the forceps, and afterwards found the scoop the best instrument for getting out the remainder, which was quite pulverised, and its total removal appeared to be interminable. I suppose from twelve to eighteen times I brought out the scoop nearly full. I then washed the bladder well out with the syringe; and, having ascertained that all the remains of the calculus were removed, I passed a piece of gum-elastic catheter through the wound into the bladder, and, having secured it in its place, lightly plugged it with lint, and was indeed glad to have my poor little suffering patient removed to bed, after undergoing an operation which lasted, I believe, for nearly half an hour, and from the effects of which he appeared dreadfully exhausted. I then administered to him an opiate

draught, with some aromatic spirits of ammonia, which I ordered to be repeated again in the evening, and to have his abdomen well fomented; and on the following morning was glad to find that he had slept for nearly four hours during the night, and was in all respects tolerably comfortable. I repeated his medicine, and ordered the free use of diluents, and in forty-three hours removed the gum-elastic tube from the wound, the walls of which I considered sufficiently covered with lymph and solidified to prevent extravasation of urine. At this time he felt some spasmodic pains in the abdomen; his tongue was white, and his skin hot; but his pulse continued good, and under ninety beats in the minute. His bowels were not relieved since the operation, but his urine was abundant, and passing off through the wound, in which he complained of little pain. I ordered him some castor oil, with aromatics, and a little syrup of poppies; and if his bowels were not then relieved, an enema to be administered, containing turpentine and tincture of asafœtida, which I found on the following day to have afforded wonderful relief, having removed from the colon a considerable quantity of scybala. From this time out he gradually improved; the wound healed up, the urine passed through the natural passage, and in a month he was discharged, cured. I have since heard of him, and been informed that he continues in perfect health.

The next case was a boy named Eugene Callaghan, aged six years. He resided in Mitchelstown, and was admitted into the Cork South Infirmary on the 15th September, 1858. His constitution did not appear to have suffered much from this disease, though the symptoms were well marked. On sounding him, the stone did not appear large, but was evidently hard, and his bladder in all respects was quite healthy.

This was a most favourable case for the operation of lithotomy, and accordingly it was the one determined on in this instance; and on the 22nd of September, after putting him under the influence of chloroform, I operated on him in exactly the same way as in the preceding case, and removed from him a uric acid calculus, weighing four drachms and a half. The operation was completed under the minute, without any accident. The patient rapidly recovered, without any unpleasant symptoms, and was discharged cured from the hospital on the 23d of October, being in hospital forty-five days.

The next case was that of a boy, named John Grant, residing in Coach-street, in this city; he was eight years old, and admitted into the South Infirmary on the 14th of October, 1858. On my first interview with him, I was deeply moved to see so miserable a creature; his worn and pallid aspect, his attenuated frame, his great sufferings, his pitiful entreaties for relief, and hopeless expression, were enough to soften the hardest heart; he was then over two years thus afflicted, and his unfortunate family pressed upon me to use any means, regardless of the danger incurred or pain inflicted, which would give a chance of releasing him from his wretched condition. In addition to the other strongly-marked symptoms of stone in the

bladder, this poor creature suffered from a constant dribbling of urine, and pain in the perineum, accompanied by a frequent bearing down; his bowels were irregular, either constipated, or he had diarrhoea, but had not for some period passed a formed evacuation; his urine was phosphatic, and hectic fever was evidently hurrying him to the grave. I endeavoured to pass the sound, which was arrested at the neck of his bladder by striking against a calculus, which seemed to be impacted there; and, on passing my finger into his rectum, I readily ascertained that such really was the case, and that it seemed to extend itself into the bladder, and to be of large size. I had a full consultation on this important case; and, though it seemed most unpromising, resolved, with the sanction of my colleagues, to try and rescue this poor creature. The lateral operation was the one resolved on, as likely to give him the best chance. I was to cut down on the stone, and then to extract it the best way I could. Previous to the operation, I used my best efforts to cheer him up, and by kindness to give him courage; also to improve his general health with nutritious food, and invigorating and soothing remedies. The decoction of Pareira seemed to have good effect in relieving the irritability and pain of his bladder, and on the 24th of October I considered him fit to undergo the operation, when he was placed on the operating table, the usual precautions having been taken, and being secured in the usual way for the lateral operation. It was impossible to get him to retain his urine, and useless to introduce a staff into the urethra. I got my finger into the rectum, and with it tilted forward the calculus, so as to make it be felt in the perineum. I then made an incision as described in the foregoing case, only, instead of striking the staff with the knife, I struck the calculus. I then introduced my finger into the wound, and ascertained how the calculus was situated; and then, withdrawing my left finger from the rectum, and introducing it into the wound, and using it as a guide for my knife, opened the urethra fairly between the bulb and prostate, and keeping it firmly against the calculus, pushed it into the bladder, cutting the left side of the prostatic portion, as in the usual operation. I then, with some difficulty, but at the same time gently, introduced my finger into the bladder, and felt the stone completely encircled by its neck, and extending far into it. I then endeavoured to grasp it with the forceps, when a small bit broke off; and I found it quite impossible to detach it from the membranes, by which it was closely and intimately surrounded. On a few moments of as calm reflection as the circumstances would permit of, I saw that my only chance of success was to push the calculus completely into the bladder, which I at once accomplished without any difficulty, but felt that it would be quite impossible to remove the stone without enlarging the wound; and as I had cut the left side as far as I could fairly do, without injuring the body of the bladder, I considered my best course was to cut the right side to the same extent, and at once did so with my blunt-pointed bistoury, without experiencing any difficulty, or

any unpleasant consequences having taken place. I then introduced the forceps into the bladder, guided by my finger, and at once delivered the stone. I then introduced a gum-elastic bougie into his bladder, as in the former case, and was happy to find that my patient had borne the operation admirably, which lasted four minutes and a half. He got some restoratives during the operation, was then placed in bed, and a suitable anodyne was administered. He was well fomented at night, and had his anodyne repeated; and on the following morning I found him tolerably well, though he had not slept much. He complained of occasional darting pains through the abdomen. I ordered him a few drops of laudanum, with a small quantity of aromatic spirits of ammonia every six hours, and four grains of Dover's powder, and three of hydrargyrum cum cretâ, to be taken at night, and to be fomented again. The urine was passing freely through the wound, in which he did not suffer much pain. On the following morning I found that he had rather a comfortable night, and slept some hours; but his bowels not being affected since the day of the operation, I ordered him a small dose of castor oil, with a little paregoric. In the course of that day I received an urgent message, to say that it was feared my stone patient was suffering from peritoneal inflammation, and in fact was in a deplorable state; when I hastened to his side, and from my former experience of such cases, expressed my opinion to our able and highly satisfactory house-surgeon, Dr. Callaghan, that I believed his ailment to be spasmodic, and dependent on scybala retained in his bowels from the pressure of the stone on the gut, as in some cases of pregnancy familiar to us all. My poor patient, from his weak and irritable state, was quite overcome from the pain; he looked dreadfully sunken, and his skin was cold and clammy; his pulse was 90, small and weak, but regular, and his tongue white and moist. He bore, and rather liked, pressure on the abdomen. Dr. Callaghan quite concurred in my opinion of his case, and we ordered him a dose of the aromatic mixture, and to have a large enema containing turpentine and tincture of assafœtida thrown up. I waited to see the result, and was happy to find masses of feculent matter come away: in fact, his bowels were in a dreadfully loaded state; it was incredible the quantity which he passed. The gum-elastic tube was removed from the wound, a poultice was applied, chicken-broth was freely administered, and he occasionally took purgatives likely to solicit the action of the bowels, and for some time scybala continued to pass off. My patient rapidly recovered, and was discharged, perfectly cured, on the 20th of November, 1858, being forty-seven days in the house.

I have but few observations to make on the foregoing cases. It is generally admitted in this country that lithotomy is a preferable operation to lithotritry for many persons. All appear to agree that the success of the operation, in a great measure, depends on not cutting beyond the prostatic portion of the bladder into the loose areolar tissue, which is sure to be followed by infiltration of urine and its dire consequences: indeed I have seen the most prolonged and

painful operations successful where the bladder was scarcely at all cut, and the calculus was delivered by dilatation of its neck. In my own practice I have been most careful in uniformly limiting my incision to the dense prostatic portion of the bladder, and to which I attribute my uniform success in a not inconsiderable number of cases which have been operated upon by me.

Peritonitis has, by many, been regarded as a frequent occurrence after this operation. I have only seen it take place as a consequence of infiltration of urine; but I have often observed that abdominal pain, dependent on scybala in the intestines, has been mistaken for it, and the patient submitted to much unnecessary and injurious treatment. I am happy to say that the old-fashioned idea of few recovering completely from the effects of this operation is now obsolete. In all the cases which I have had, the patients were restored to perfect health after the calculus was removed, and to my knowledge never suffered any ill effects from it to the present time.

DR. FINN, on behalf of Dr. Rountree, of Ballincollig, exhibited pathological specimens, illustrative of following case, and read the particulars as communicated:—

*Chronic Pleuritis, with Effusion.*—The patient was a young artillery driver, named Barnes, stout and rather muscular, who was brought to the military hospital at Ballincollig on the evening of the 6th December, 1860, apparently in a state of great suffering, from an acute pain in his left side, which was much aggravated by his short and difficult breathing. His countenance was troubled and restless, lips and tongue purplish; he was chilly, and anxious to lie down; his hands and extremities were cold and livid; tongue was coated; pulse hard, quick, and oppressed; he endeavoured to restrain a short cough which came on occasionally, to avoid the pain it caused him. I learned from his comrades that the proximate cause of the present attack was in consequence of having incautiously slept on a heap of wet horse-litter in the open air for some time during the forenoon, which was bitterly cold and damp, and this after a debauch on the previous evening. On awaking, he found himself extremely ill, and shivering with cold and pain. He was not brought to the hospital until late in the afternoon. On examination of the thorax, I found on percussion the left side dull all over, from the clavicle to the costal cartilages; the rubbing, friction sound was also perceptible; the respiration was altogether tubular at the right side, there was besides dulness on percussion on the infra, mammary and dorsal regions; confused sonorous and sibilant râles being heard also. There was but little expectoration, following a hard painful cough; the former was glairy and adhesive. His decubitus was sometimes on the right side with his shoulders raised, turning occasionally on his back, and for short periods on the affected side. After being made more comfortable, and suitable remedies administered, he appeared to have rallied; and on the following day his countenance became clearer, and

the other symptoms seemingly mitigated; the dulness of the side affected was still impenetrable; the friction sound was scarcely heard, but ægophony was evident in the infrascapular regions; the sputa was not increased in quantity, but was quite characteristic; the breathing was still difficult and abdominal, but the immobility of the left intercostals was more apparent now, as he was less restless. On the approach of evening, after a disturbed sleep, the dangerous symptoms of dyspnoea reappeared; his face and extremities becoming livid, and his breathing hurried and gasping. It was not without much difficulty that I succeeded in restoring him from this condition. He continued much easier, without any remarkable remission of the morbid symptoms, until the fourth day, when he had another very alarming return of dyspnoea, with lividity of countenance, failure of pulse, jactitation, and delirium. He was again relieved from this state, and lingered on for three days longer, with frequent returns of those dangerous attacks, generally following sleep, from which he started in delirious terror, gasping like one suffocating, sometimes rallying, speaking rationally, taking nourishment, and the pains and active symptoms again returning with severity, when at last he sank gradually, without much suffering.

I had not an opportunity of learning the early medical history of this man's case correctly, and could only collect from himself, or some of his comrades, some imperfect particulars.—That, some five years before he had, while on his march through the interior of Ireland, a severe attack of inflammation of his lungs, and had to be left behind in charge of a private practitioner; that he had a relapse of the same, and was near dying of it subsequently; he was also in hospital on three or four occasions more in other places, but that for the last three years he was able to perform all his regimental duties remarkably well, attending to horses, &c.; that he was very short-winded, and not equal to violent exertions; he drank occasionally, when he could get it; and the evening before the last fatal illness he became very tipsy in a public-house, and amused himself by singing and dancing; it appears also that he felt himself ill and uncomfortable some days before this, but did not come to hospital, thinking that it would wear away.

*Post-mortem Examination.*—The body was well-formed and muscular; there was no remarkable difference apparent between the size of the two sides of the thorax, nor other external signs of chronic disease. On dividing the costal cartilages of left side, serous fluid came welling over, and I found the cavity entirely filled to overflowing with sero-purulent effusion, shreds of lymph being interspersed, and granulations of organised lymph studding thickly the costal sides of the cavity in various patches; the lung, much collapsed, and intensely carnified, was pushed over towards the mediastinum, and protruded into the right cavity of the thorax, and was attached in various parts by numerous bands of false membrane to the costal pleura, particularly at the superior part of lung in the subclavian region, interlacing and forming there a kind of cellular structure;

the heart and large vessels were not much displaced from their natural position; the former was large, but healthy; there was much congestion of the right lung, particularly at the base and posterior portions of the lobes, which were of a dark-brown colour; the liver was much enlarged and congested also.

This case furnishes a remarkable instance of the possibility of a person existing for so long a period without any extraordinary discomfort, although he carried within his thorax a copious effusion of fluid, having a collapsed lung, of little use for the purposes of life, with other morbid lesions attendant on this state. Still he ate, drank, and enjoyed himself like other men, and looked well and in good condition, and attended regularly to all his regimental duties. However, when an acute attack of illness occurred, brought on by his own imprudence, he readily succumbed, his chances being lessened by the consequent congestion of his right lung, there remaining but a small portion sufficiently sound to carry on the functions of breathing.

#### FEBRUARY 13.

PROFESSOR O'CONNOR in the Chair.

*Case of Non-Maculated Fever, terminating fatally, with Diphtheritic Symptoms.*—By DR. CUMMINS.

On the 30th of April, 1858, when ague was very prevalent in the neighbourhood, I was called to attend Mrs. I, a full plethoric woman of forty-five, who was suffering from most intense head-ache and high continued fever, alternating with occasional diaphoresis. She had not closed an eye for several nights.

I immediately prescribed a draught containing tartar-emetic and opium, which produced copious diaphoresis and some sleep, with great relief of the head-ache, and other symptoms. After a few days, regular tertian ague became developed, which rapidly yielded to quinia, and she was restored to perfect health. On the 28th of May, 1860, I was again called to her, and found her suffering from the same symptoms, the head-ache especially being much complained of, while the fever, with pulse of 80, continued uninterruptedly; there was a partial exacerbation every second day.

I treated her at first with diaphoretics, and a few leeches behind the ears, cold to the head, &c., without relief.

I then tried small doses of quinia, which rather increased the head-ache, without doing good otherwise. She was sponged twice daily with tepid vinegar and water, and she got an occasional opiate with tartar emetic or James' powder, without which she never got sleep. Tongue continued clean, and pulse 80.

On the seventeenth day she became delirious when left to herself, but perfectly rational when roused.

On the nineteenth day there was an increase of the delirium, with tympanitis, and the pulse had risen to 100, the tongue con-

tinuing as before, *perfectly natural*. A draught of oil and turpentine somewhat relieved the tympanitis.

Twentieth day.—She seemed rather better, but the head-ache was much complained of. I then had the head shaved, and gave her six oz. of wine in the twenty-four hours.

Twenty-first day.—There was a tendency to coma, with some ster-tor when left to herself. The urine had to be drawn off with a catheter twice a day, the secretion being perfectly natural in quantity and quality. She can be roused, but soon lapses again into a drowsy state. No cough or dyspnœa, but two or three expectorations were tinged with blood. Blister to back of neck and head. Wine 12oz.

Twenty-second day.—Still in the same drowsy state, but hands are continually in motion, picking the bed-clothes, and there is constant muttering delirium; still can be roused to perfect consciousness. Pulse 90, natural in force, and fulness. The secretions from bowels and kidneys are perfectly natural, and the tongue exhibits the most perfect conditions of health. No return of bloody expectoration, but there is slight tympanitis. Dr. W. Townsend saw her with me to-day, and coincided in my most unfavourable prognosis.

On the 23d day I was very unwell myself, with feverish symptoms, and was not able to visit her in the morning. Dr. Townsend, however, reported to me that the tympanitis was very great, but that in other respects she continued in much the same state.

We agreed to give her three drachms of castor oil, with a drachm and a half of turpentine in a two-ounce draught. This she took about 11½ A.M., and, immediately after swallowing it, complained of its burning her throat. At noon the throat was much complained of, and the symptoms were described to me as so urgent that I got up and went to her. I found her sitting up in bed, supported by pillows, perfectly rational, with a thready pulse of 86, suffering from most intense dyspnœa, and a large secretion of mucus, mixed with small patches of a white exudation, rattling in her throat, and flowing continually from her mouth.

The dyspnœa was so urgent that I found it impossible to make a satisfactory examination of her throat, more especially as vision was obscured by the copious secretion.

Intellect was perfectly clear; the pulse never rose above 86, and she was almost quite unable to swallow.

At six o'clock, P.M., on attempting to swallow some brandy-and-water, she became asphyxiated, and died in a few moments.

My treatment during the six hours which this last severe attack continued was directed to endeavouring to administer stimulants, and applying warmth to extremities, cardia, &c., and counter-irritants to throat externally.

I was so very ill myself at the time, and for several days after, that I was unable to make any post-mortem examination of the throat; but I find it difficult to account for the sudden invasion of the fatal symptoms and the rapid course they ran, except on the



supposition that diphtheria was their cause; and the antecedent typhoid debility, which took away from the patient all chance of wrestling with the fatal grasp of so formidable a complication, the reason why it ran so rapid a course.

It is worthy of note, that two days before there had been some sanguineous expectoration, which probably came from the throat, as there was no cough that would account for it otherwise. Did the throat symptoms commence then, and remain in abeyance until the draught of turpentine and oil, acting as a local irritant, called it into more energetic action? I think such must have been the case, notwithstanding the entire absence of other symptoms referred to that part during the intervening period.

This case recalled to my memory a clinical lecture delivered by Dr. Corrigan last February, and I have been fortunate enough to find it in the sixth volume of the Dublin Hospital Gazette. He takes as his text a case of non-maculated typhus, which occurred in the Hardwicke Fever Hospital, and which, on the sixteenth day, became complicated with diphtheria, which ran a fatal course in about twenty-four hours.

Dr. C., remarking on it in his usual lucid style, makes the following observations, which, I think, apply equally to the case before us:—

“Was the fever under which Mrs. I. laboured for several days one peculiar to diphtheria, or was it merely one of non-maculated typhus—diphtheria terminating life as an accompaniment or sequel—as we frequently see in other instances when local disease is prevalent, having a tendency to be developed towards the termination of any of our depressing fevers? The latter was probably the case in the instance before us; but this cannot diminish our fear about the disease—whether as an idiopathic disease, or as a sequel of fever, there is much to be dreaded about it.”

This was one of the most treacherous cases I ever witnessed, and as such is remarkably interesting; the symptoms setting in of somewhat an aguish character, the pulse being little increased in frequency, the tongue and all the secretions continuing natural throughout its entire course, while at the same time the lesion of the cerebro-spinal nervous system became more and more profound from day to day, until near the fatal termination, the case presenting a group of symptoms contrasting strangely with each other.

Drs. Graves and Stokes long ago pointed out that unusual symptoms in typhus, even when favourable in themselves, should be regarded with suspicion, and this case most remarkably demonstrates the correctness of their observations.

*Pneumonia—its Treatment.* By T. W. BELCHER, M. A. and M. B., Oxon. and Dublin, L. K. and Q. Coll. Phys., Ireland.

The object of the details in this paper may be briefly stated as an attempt to determine the best method of treatment in certain acute diseases, of which pneumonia is selected as an example.

During the half-year ending December 31, 1860, fifteen cases of this disease were treated in Dr. W. C. Townsend's division of the Cork Union Hospital. Of these nine have been discharged cured, five remain as convalescents, and *one* died.

The fatal case was that of a person, who, having for many years laboured under complicated disease of the respiratory organs, contracted pneumonia in a state of weakness and prostration, so that his chance of recovery was small. At various times, however, he rallied, and the disease in a measure subsided; eventually he died, and his case, which was carefully noted by Dr. Bernard, was, with the result of a post-mortem examination, exhibiting fetid abscess of the lung, read by that gentleman before this society at an early period in the present session.

If this case be removed from the category of "cases of pneumonia"—as the writer considers it ought—it becomes evident that the remaining fourteen all resulted in cure.

As the object of this paper is merely to determine the best mode of treatment, and not to give a complete history, *three* cases, noted in *extenso* by Dr. W. C. Townsend, shall, for the present purpose, be exhibited in tabular synopsis.—[See Table on the next page.]

To show, in *detail*, the application of the treatment adopted, one of many may be quoted:—

Daniel McCarthy, aged 37, married, a labourer, of temperate habits, admitted on the evening of December 4th, a fortnight after his discharge from the fever hospital, where he had erysipelas of the face.

December 5th. Complains of headache and cough, with pain in the chest; tongue white, red at the edges; skin hot and dry; pulse 92, and weak; respiration 34 in a minute.

On examination, the left lung was found *generally* solid, the excepted portion being very small: bronchial respiration was distinctly heard all over the solidified part; crepitus in the remainder.

*Treatment.*—Ordered 5 grains of Dover's powder every four hours; 8 ounces of wine; a blister to back and front of left lung; also beef tea and diluents.

6th. He spent a sleepless night; the blisters rose well, and he feels better; pulse 120, and weak; respiration 42; expectoration copious, and mucous; dyspnoea greatly relieved.

A little air passes through the upper part of the left lung.

Urine full of chlorides.

*Treatment.*—Continue Dover's powder and wine.

7th. Slept well; expectoration slightly rusty; pulse 140; respiration 32; crepitus continues.

*Treatment.*—Continue as yesterday.

8th. Pulse 120; respiration 30; other symptoms same as yesterday.

*Treatment.*—Continue powders and wine.

9th. Same as yesterday.

1.—Daniel O'Connell, aged 40, farm labourer.	Good.	Probably sober.	Middle and inferior posterior part of right lung, and afterwards upper part of left.	Solidified.	None.	Chlorides absent.	Pain in chest; cough; expectoration; pulse respiration; skin; tongue; general appearance.	Various results of auscultation and percussion, particularly, puerile respiration, crepitus; bronchial breathing; mucous roushus.	Cured.	13
2.—Michael M'Carthy, aged 40, labourer.	Good.	No report.	Whole of right lung posteriorly.	Lowest part solid, middle and uppermost permost engorged.	Recovered from an injury of right side shortly before admission.	Chlorides present, but in small quantity.	Ditto.	Ditto.	Cured.	16
3.—Patrick Noonan, aged 18, labourer.	Good.	Sober.	Middle and lowest lobes of right lung posteriorly.	Solidified.	None.	Chlorides present and lithated.	Ditto.	Ditto.	Cured.	13

Blisters to part affected, Dover's powder, and mercury with chalk, of each, 5 grains every 4 hours (on first day); afterwards, omit mercurial, but continue Dover's powder for several following days, until near the close, when 5 grs. were given every eight hours; beef-tea and arrow-root; wine, at first, 4 ounces, increased to 6 ounces in the 24 hours, then decreased to 2 ounces, all given in small quantities.

Dover's powder, 5 grains, with nitrate of potash, 10 grains every four hours, blister to part affected; beef-tea; wine 4 ounces, increased to 6 ounces, and afterwards reduced to 4 ounces.

12 leeches to part affected, and cupping afterwards; Dover's powder, 5 grs. every 4 hours; blisters; diluents beef-tea; arrow-root; 8 ounces of wine, reduced to 6 oz., then to 4 ounces.

10th. Slept tolerably well; pulse 116; respiration 28; sputa viscid, and slightly tinged with blood; tongue cleaning; sounds clearer all over the chest.

*Treatment.*—Continue as before.

11th. Pulse 112; respiration 28; sputa improved, as also the pulmonic sounds.

*Treatment.*—Reduce 8 ounces of wine to 6 ounces; and Dover's powder from one every four to one every *eight* hours.

12th. Much better in every respect.

*Result.*—In a very few days this man became convalescent; the wine and Dover's powder being diminished as he daily improved; and in a comparatively short time he regained his health, without any apparent diminution in his ordinary strength.

It is quite obvious that the treatment generally pursued in these cases has been most successful as to the result; while it is also manifest that—with just sufficient exception to prove the rule—blood-letting, tartar-emetic in large quantities, and mercury—the sheet-anchors of orthodox antiphlogistic treatment—have been discarded; and not only so, but, as was formerly supposed its mortal foe, alcohol has, with good effect, been to some extent substituted for them.

The remedies used were intended to promote the free action of the skin, kidneys, and, to a lesser extent, that of the intestinal mucous membrane. Constant local stimulation was employed, while the powers of life were upheld by suitable nourishment, easy of assimilation, and calculated to compensate for the loss of muscular and nervous tissue occasioned by the wasting character of the inflammatory process\*.

All this proceeds on the presumption, that instead of the inflammation being attended with undue exaltation of the vital forces, it is a disease of nutrition, depressing the vital powers, and hence requiring support; and, though it doubtless appears plausible to assert that the want of appetite, weakness, and wasting, occurring in this disease, are natural processes, forming “a set-off” against the undue exaltation of the vital forces, yet the more natural explanation of these phenomena is, that nutrition being arrested, these results plainly follow; and the obvious tendency of the treatment should be to uphold the vital powers by such food as can be easily assimilated. In this category alcoholic *food* stands; and medical men should not countenance the very prevalent opinion, that as a mere stimulant, and in that light only, should this important therapeutical agent be regarded.

On the principles above stated, it is manifest that the ancient and orthodox practice of general bleeding is inadmissible; and the old statement—that inflammation can be cut short by it—is, in the opinion of many practical men, a statement incapable of proof.

\* See Todd's Clinical Lectures, p. 230, Beale's Edition, 1861.

† J.H. Bennett's Clinical Medicine, Third Edition, p. 282.

The use of mercury was discarded, because its vaunted advantages, in cases such as these, rest on mere theorising; while, in many instances of recovery under this part of the antiphlogistic system, it may be truly said the cure was worse than the disease.

Moreover, in the instances already enumerated, the patients recovered without it; so that, at best, it was unnecessary.

In the face of such evidence, is it not illogical and unfit that medical men should argue against a modern *practice*, because it goes to upset an old and time-honoured theory; and, while they recognise the importance of the pathological and physiological investigations of late years, yet steadily refuse to admit it in practice, thus making the discreditable avowal that medical knowledge and practice cannot agree?

One valid argument exists, that of experience. Let any one treat similar cases to those above-mentioned in a similar manner, and then declare the result.

Lord Bacon says,<sup>a</sup> "Every medicine (remedy) is an innovation;" and he adds, "It is true that what is settled by custom, though it be not good yet at least it is fit; and those things which have long gone together are, as it were, confederate within themselves; whereas, new things piece not so well; but, though they help by their utility, yet they trouble by their inconformity; besides, they are like strangers, more admired and less favoured."

Professor Hughes Bennett<sup>b</sup> enters largely into the statistics of various modes of treatment pursued in pneumonia; the following is a short summary of his statements:—

Result of vigorous antiphlogistic treatment, mortality, 1 in 3. Treatment by tartar-emetic, according to Rasori, and more recently, Dietl, 1 in 5; according to Lænnec, 1 in 10. Result of moderate bleedings, according to Grisolle, 1 in 6½. Result of dietetic treatment, with occasional bleedings and emetics in severe cases, Skoda, 1 in 7; Dietl, 1 in 13. Result of treatment directed to forward the natural progress of the disease, J. Hughes Bennett, 1 in 26.

There can be little doubt of the great evils attendant on over-stimulation in such cases as those now referred to; but practical experience has shown, that while under the antiphlogistic treatment, 1 case in 3 died; under treatment by large doses of tartar-emetic, 1 in 5 died; the result of more moderate bleedings was 1 death in 7; but a course of treatment directed to further the natural progress of the disease gives but 1 death in 26.<sup>c</sup>

The cases treated in the Union Hospital were those of persons of various ages, and in almost every variety of humble life. The average age was 41 years, the lowest 9, and the highest 66; thus showing that the plan adopted is suited to every condition of the disease.

The presence or absence of chlorides in the urine, coincidentally

<sup>a</sup> Essay on Innovations.

<sup>b</sup> Clinical Medicine, pp. 289, &c.

<sup>c</sup> J. Hughes Bennett, *op. cit.*, p. 682.

with an opposite state in the diseased sputa, is insisted on by modern writers as a symptom of pneumonia of much importance, and only requires statistical observation to make it very valuable as a source of information.

The undoubted short period of convalescence in the cases before us is also deserving of consideration.

If, indeed, as has been asserted, the old theory of inflammation is incompatible with our present state of scientific knowledge, and if consequently the practice founded on such a theory be unsound, should we not adopt a practice which commends itself to all moderate men as simple in the using, successful in the result, and tending strongly to follow out the old Roman maxim—

“*Salus populi suprema lex.*”

Lest it should be imagined that the treatment pursued in the cases detailed above is inapplicable to the better classes, and suitable only to the lowest conditioned of our population, I venture to append the following observations of Dr. W. C. Townsend, made during the discussion on the previous paper. These remarks serve also to elicit the views of one well calculated to form a practical opinion on the subject, of one who is a practitioner of long standing, and for several years an hospital physician in this city. To his kindness I am indebted for the suggestion that these cases should be brought before the profession, and for the ample opportunities afforded me for doing so. “For the last two years I have treated in private, as well as in hospital practice, a large number of patients labouring under pneumonia: in these cases I have not used a particle of calomel or any other mercurial, except as a purgative, and I have rarely, if ever, lost a patient.”

With reference to the cases detailed in the paper now under discussion, I would not advocate an extreme principle of overstimulation, which, as far as my experience goes, I think quite unnecessary; but I would submit for the consideration of the profession, whether the old system of large bleedings, mercurialization, and keeping patients in the lowest possible state, may not have been carried too far; whether a different and more moderate course might not be adopted with great advantage; such course having in view the supporting the system, while the disease was being eliminated by the natural processes?

If we are to consider what is called “idiopathic pneumonia” as the result of a morbid process, disturbing the circulation, arresting nutrition, and necessarily lowering the powers of life, it may be well to ask ourselves whether a plan of treatment which, on the principle of subduing inflammation, has for its aim and end a further lowering of the system, may not be mischievous? Whether the depression of the vital powers, thus induced, must not be injurious, and very frequently terminate in a tedious convalescence, leaving the patient an easy prey to some other morbid tendency

till then lying dormant, but which, as long as the constitutional powers were strong, would never be called into action.

I very much fear that we are too apt to adhere to long established opinions, ideas, and systems of treatment. They, as it were, grow with our growth, and strengthen with our strength; the slightest deviation from them is considered an innovation. Would it not at least be worthy of consideration whether we do not trust over-much to therapeutical agents in controlling disease, and too little to the natural powers of the system?

I must confess my experience induces me to adopt the views of those who hold that more advantage is obtainable from assisting and controlling the natural processes, than can be obtained from adopting those extreme measures which have in view the presumed possibility of cutting short an inflammation or extinguishing a fever.

There is no denying the fact that our views are every day, insensibly to ourselves, changing; no sane person now either bleeds or mercurializes to the same extent in pneumonia, as was the common practice twenty years ago; and no matter how strongly the moderate plan of treatment, by which I mean moderate support, and the careful use of stimulants so as to uphold the system, while the disease is being eliminated, may be abused, it is now very generally adopted even by those very persons who decry it.

## FEBRUARY 27.

PROFESSOR O'CONNOR in the Chair.

DR. WILLIAM P. BERNARD related the following case of *Double Pneumonia, treated by Stimulants*.

Robert Williams, aged thirty-eight, a plasterer of intemperate habits, was admitted into the Union Hospital, under the care of Dr. Townsend, on the 21st of January, 1861, suffering from frequent rigors, accompanied by dyspnoea and pain in the chest.

Though emaciated and very debilitated, he states he never suffered from ill health until three days before his admission, when, early in the day (and while at his work), he felt a dizziness in his head, followed in the evening by a very severe rigor, which lasted from three to four hours, and again attacked him, but with less severity, on the following morning.

January 23, 1, P.M.—Is just recovering from a rigor, which the nurse describes very severe; his teeth chattering and his whole body trembling during the paroxysm; skin moist; tongue very hot, dry, tremulous, and covered with a brown crust in the centre, the edges, and tip red; bowels constipated; urine scanty and high-coloured; pulse and respiration very rapid; no head-ache; complains very much of pain in both sides of chest, from the nipple downwards; this and the dyspnoea prevent his remaining in one position for many minutes at a time, and he chiefly lies on his back; troublesome

cough; sputa scanty, rusty, and tenacious; marked dulness on percussion over root and base of both lungs posteriorly; bronchial breathing and broncophony both very well marked. These symptoms, though present over base of lungs anteriorly, are very much modified in degree.

Treatment. Bowels to be relieved by house medicine, after which five grains of compound ipecacuan powder, to be taken every four hours; to have eight oz. of wine until next visit.

January 24. Bowels freely relieved since last report; cough troublesome during the night, which prevented his getting sleep; tongue moist, but still hot, and very coated; pulse 100, regular and feeble; feels freer from pain, but no relief from the dyspnoea; expectoration more profuse; rusty, with blood-streaks on the surface; very tenacious, adhering firmly to the bottom and sides of the vessel.

To have turpentine stupes over base of both lungs posteriorly, after which large blisters to be applied. Continue treatment, giving him, in addition, half a pint of beef-tea.

January 25. Feels easier since the blisters have been dressed. Continue treatment.

January 26th. Slept better last night; tongue still very furred; pulse 100, but more feeble than hitherto; expectoration very rusty and puriform, but less tenacious, and not streaked; moist crepitation over anterior and inferior portion of both lungs, but more aggravated at right side; increased resonance over base of both lungs posteriorly.

Continue powders; wine to be increased to 12oz., and beef-tea to one pint. Tested the urine for chlorides, and found them largely present.

27th. But little change since last report.

28th. Slept soundly; pulse 88, extremely feeble; tongue cleaning; expectoration more tenacious, and freer from tinge and pus; moist crepitation over anterior inferior portion of right lung; respiration clearer over corresponding portion of left lung; moist crepitation over base and inferior portion of both lungs posteriorly. Continue treatment.

30th. Much improved since last report; wine to be reduced to 10oz., and the powder to be given every eight hours.

31st. Feels very much better; sleeps well; tongue cool, clean, and moist; skin soft; pulse 60, full and regular; expectoration copious and purulent, as if from an abscess; respiration very much improved. Continue beef-tea; wine reduced to 8oz., and the Dover's powder be taken only twice daily.

February 1st. Continues to improve.

2nd. Feels as easy and almost as well as before his illness; is anxious for a chop; wine stopped, and to have 6oz. of meat and a pint of porter daily, with bread *ad libitum*; his powders to be continued only at night. From this period he continued to progress rapidly, and at present enjoys good health.



Perhaps the chief point of interest in the foregoing case is the treatment adopted, and which has proved so successful in this instance; which is but one of many similar cases so treated in the hospital with a like result. One of the agreeable features in it is the rapid convalescence, which could hardly be expected from the severity of the case, and its tolerably frequent reverses. It at least shows that the stimulant plan of treatment is worthy an impartial and complete investigation. Who more suited to conduct such a research than those connected with our hospitals, where so many opportunities of comparison and complete supervision are to be had, and which could not be carried out with anything approaching the same certainty in private practice.

PROFESSOR O'CONNOR read the following:—

*A Case of Monstrosity; deficiency of the Abdominal Walls, of the Cartilages of the Ribs, and a part of the Diaphragm; Hernia of the Heart.*—Mrs. O. S. had symptoms of miscarriage between the seventh and eighth month of pregnancy. Slight hemorrhage continued for two days at intervals, at the end of which time labour-pains set in, and she was speedily delivered. To my astonishment, I found the abdomen of the child attached to the mother by a large bag, resembling the membranes of the uterus. Feeling through this bag I found the funis enclosed within; and while in the act of tying it, the placenta came away. The bag, which appeared to be the peritoneum of the child, was attached to the placenta, and contained the funis, which was not more than four inches long; and accordingly the abdomen of the child must have lain against the placenta whilst in utero, and produced the deformity I am about to describe. The walls of the abdomen were entirely absent, and the abdominal organs were seen hanging loosely, not being bound down by any covering. The liver and spleen were particularly pendant. The lower part of the sternum and cartilages of the adjoining ribs were also deficient, and the heart was quite visible, lying on a line anterior to the walls of the chest. The child never made any effort at respiration, which would have been impossible from the state of the diaphragm, but the heart was pulsating with considerable force. Having observed it for a quarter of an hour, and seeing the pulsations become weaker, and knowing there was no possible chance of resuscitating the child, I opened the pericardium, when I found at once the action of the heart become stronger. It beat then and for some time twenty-four times in a minute. If it grew weak, I touched it with a little spirits of harts-horn, and it became at once more excited; touching it with the finger also made it anticipate its time of action. I placed my finger on the aorta to retard the blood in its passage from the heart, and there was double systole, as we find in valvular diseases. In looking at this strange phenomenon, I endeavoured to realize to myself what the convictions of a person free from preconceived ideas would be as to the motions of the heart. After systole the heart lay in

a state of apparent collapse, from which it suddenly rose up as if by a convulsive shock, the most apparent effect of which was a visible swelling up of the heart, caused of course by the contraction of the vertical diameter, and the passage of the wave of blood through the ventricle. Any one not having studied the subject would have thought that systole was the moment in which the heart was filled, not emptied. During systole I could not distinguish a difference between the auricular and ventricular contraction, still the eye was satisfied that all parts of the heart did not act at the very same time; but occasionally, when the action of the heart became very feeble, I saw as if a vermicular movement of the right auricle, which did not reach the ventricle. My impression also was that there was a slight filling of the ventricle during diastole; and indeed I cannot imagine why this should not be the case at all times, when we know there is a continuous stream of blood always arriving at the heart from the veins, and there is nothing in the auriculo-ventricular valves during diastole or rest to prevent the blood passing onward to the ventricle.

The lady was delivered about eight o'clock, and I continued to watch the motions of the heart till ten o'clock. I instructed the nurse to observe the time at which it would cease, which she reported to me next morning was at half-past ten, two hours and a half after delivery. I regret this rare opportunity was not presented to some one more competent than I was to derive positive knowledge on this singularly difficult subject of the sounds and movements of the heart.

MARCH 13, 1861.

DR. TANNER, PRESIDENT, in the Chair.

*Thoracic Aneurism; Dilatation and slight Hypertrophy of Heart.*—

Dr. Finn\* having expressed his acknowledgments to Dr. Golding, Assistant-Physician to the North Infirmary, for assistance rendered in the *post-mortem* examination, exhibited morbid specimens of the above, and communicated the following particulars:—

Constable Denis Phelan, aged 51, of stout stature and tall conformation, was admitted into the North Infirmary on the 8th February, 1861, labouring under cough and hoarseness, which symptoms were reported to have commenced about six weeks previous to his admission to hospital. He had enjoyed habitual good health until January, 1858, when he fell from a height of fifty feet, whilst endeavouring to extinguish a fire. On this occasion he received some injuries of the chest and abdomen, for which he had been for some time under surgical treatment in the North Infirmary, and subsequently in Steevens' Hospital, Dublin. Since that period his health had never been so good as previously; he was more susceptible to the influence of cold; and exercise, however moderate, occasioned a shortness of breathing, from which he never suffered at any former period. Symptoms on the occasion of his admission—

\* In Dr. Finn's paper, at p. 234, for "nitrate," read "carbonate" of bismuth.

Pulse 54; tongue clean; regular habit of body; appetite good; slight cough and hoarseness. He experienced some difficulty of deglutition, which applied equally to liquids and solids, and referred the source of the difficulty to a point corresponding with the upper third of sternum. Physical signs—Slight prominence of the upper part of the sternum to the right of the mesial line; percussion-sound normal; respiration stridulous; but auscultation did not suggest the existence of an increased secretion from the air-passages. The sounds of the heart presented no abnormality in their character. In the course of the ascending aorta and arch, a loud double sound was heard, loudest at the prominence of sternum, above noticed; but at no point was either sound replaced by a murmur.

On the 1st March his expectoration was reported to have been slightly tinged with blood; he died quite suddenly at noon, having previously expectorated about two pints of blood.

*Autopsy* took place about twenty-four hours after death. A very large quantity of fatty matter was deposited in the anterior mediastinum. The pericardium contained about two ounces of serum. The heart was both dilated and hypertrophied, and its surface generally was invested by a layer of adipose tissue, which presented considerable density near the septum cordis. The muscular element of the heart was pale, and appeared to participate in the adipose metamorphosis. The valvular structures were free from disease. The aorta was dilated in the first and second parts of its course. An aneurism of about the size of an orange involved the right lateral and anterior aspects of the vessel (the latter to a moderate extent), the points against which the blood-wave impinges with its maximum force. It commenced about an inch below the innominate, and extended thence to near the origin of the left carotid. The interior of the artery presented a high degree of vascularity, and exposed a surface rendered extremely rough and irregular by the intrusion of atheromatous and calcareous deposit. The sac contained a large quantity of coagulum, which, though laminated, was not decolorized. The rupture took place at the distal end of the sac by a small opening into the central and anterior portion of the trachea. Had the patient survived for a short time, a second opening would have taken place into the contiguous portion of the œsophagus, which presented an advanced stage of ulceration. Both lungs were engorged with blood.

*Remarks.*—The diagnosis was facilitated in this instance by the dysphagia (already referred to) and the stridulous breathing; by the former more especially. The other symptoms were so obscure as scarcely to warrant an affirmative conclusion. The visual phenomena, noticed by Dr. Gairdner (Edinburgh), in connexion with thoracic aneurism, were not observed, though the centripetal tendency of the distal portion of the sac must have involved pressure more or less direct on the lower cervical ganglion. In two other cases of thoracic aneurism, at the same time under treatment in

hospital, no irregularity was observed in the character of the pupils, though examination had been frequently made for the purpose of ascertaining the fact.

*On Excision of Joints.* By J. E. CURTIS, Jun., Surgeon, South Charitable Infirmary.

MR. PRESIDENT AND GENTLEMEN,—In all our large hospitals and workhouses we have every day presented to our notice formidable and intractable cases of joint disease where treatment has completely failed; and where the case arrives at that point, when the experience and judgment of the surgeon are tested in the highest degree, where the life of the patient, in all probability, rests on the course he may adopt,—whether he will deprive the patient of his limb, or still trust to treatment, and the effects of nature. Another choice is still open to him, whereby a useful limb may be saved, as well as the patient's life. I allude to excision of the joint.

In coming to a decision as to his course of proceeding, he will have to depend on his own experience, judgment, and the recorded experience of others, which records, if carefully made, will be of the greatest use to him on such a trying occasion; and thinking it the duty of every surgeon to add his mite to such a record makes me now, gentlemen, introduce the following case (successfully operated on by our President, Surgeon Tanner) to your notice, as well as to afford a subject for discussion at this meeting. Before reading the daily notes carefully taken by myself, I beg to preface this by a short history of the operation, and its statistics.

I believe the honour of originating the operation of excision of the knee-joint first lay with Mr. Henry Park, of Liverpool, in the year 1781; his patient was a sailor, and had suffered for ten years from disease of the knee; and at the end of twelve months after the operation, this man was so far recovered that he was able to proceed to sea, and make several voyages, and performed all the duties of a seaman.

From this time up to the year 1830, we have had reported eleven or twelve cases operated on by Mülder, Crampton, and Syme, and of these five were successful, one partially so, and five failed.

Mr. Syme, who, at that time carried great weight with the profession, wrote against the operation; it was abandoned for twenty years. Again, in the year 1850, we are indebted to Mr. Ferguson for the reintroduction of this operation, and he was followed by many others, more particularly by Surgeon Butcher, of Dublin (who has added much to conservative surgery), by recording a series of cases occurring between the month of July, 1850, and December, 1854, in which he mentions thirty-one cases, five only proving fatal; again, from the month of December, 1854, to December, 1856, he records fifty-one cases, with only nine deaths.

Now, gentlemen, as far as I can learn, the mortality in amputation of the thigh is about one in three; and if in an operation which has been so very frequently performed (I mean amputation of the thigh),

the mortality is so great, I think great praise is due to those men who have introduced so excellent an operation as excision, attended with so little mortality, and leaving a useful limb.

I will now read for you an account of the case performed on at the South Charitable Infirmary on the 26th of March, 1860.

John Ahern, aged 20 years, farm labourer, was admitted into hospital on the 22nd of February, 1860. As far as he can tell, his previous history was as follows:—That about five years previously he perceived a slight swelling in his left knee-joint, which gradually increased for six months. He can give no reason for the swelling, the joint (so far as he knows), not having received any injury; sinuses formed, and a thin whey-like fluid exuded from it; what little pain that was hitherto in the joint ceased when this fluid appeared; at this time he could do his daily work, and walk pretty well, and the joint was not stiff; the openings in a short time closed, but reappeared in twelve months, and sinuses again formed in four places; the stiffness increased after this attack; the sinuses never healed up to the present time; however, he was able to continue his farming work up to six months ago, and from that date he got gradually worse.

On admittance into hospital a careful examination was made: the left knee-joint was found to be enlarged and rounded, with a semi-elastic swelling occupying the interstice of the joint. The limb was semi-flexed; he allowed slight passive motion in the joint without complaining of much pain, but the joint daily became more painful and tender to the touch, and the slightest motion gave him the greatest pain; he had a strumous aspect, and appeared of a delicate constitution; the integuments covering the joint were of a white colour; there were four sinuses, two at either side of the joint; he has two brothers and two sisters, all of whom are healthy, as also his father and mother.

After being in hospital some time, his health gradually began to give way; Surgeon Tanner, under whose care the patient was, came to the conclusion that it was in every way a fit case for excision; and communicating the fact to his colleagues and many experienced surgeons, who were also of the same opinion, he determined to excise the joint, and operated on Monday the 26th of March, 1860, before a large assemblage of medical men.

The operation 10 A. M.—The patient being placed on the table, great difficulty was experienced in getting him under the effects of chloroform, although he evinced great desire that it should. The operation was commenced by a semi-circular incision from without inwards, running beneath the patella, carried through the ligamentum patellæ and soft structures at either side of it. The flap thus made, and containing the patella, was turned upwards and freed to some extent; the lateral ligaments were then divided, the leg forcibly flexed, the crucial ligament cut through, and the joint dislocated. The under surface of the patella was scraped, it showing some signs of disease, the tibia was forced beneath the condyles of the

femur, and little less than one inch of the femur sawn off, the tibia acting as a guard to the vessels and soft parts; then the tibia was brought forward on the femur, and sawn off to the extent of three-fourths of an inch. There was some little difficulty, and some force had to be used in getting the bones in apposition; the leg was straightened, the flap drawn forward and three stitches put in it; two small arteries were only required to be tied; very little blood lost during the operation, but a free oozing of blood continued for some time from the smaller vessels; the limb being bandaged, a strip of thick pasteboard placed on each side of the joint and secured, the limb was put into a box-splint. At this time the patient required a large amount of stimulants, which had but little effect. On examining the head of the bones, the cartilages were found to be destroyed on both bones, as also the under surface of patella.

7 P. M., same day.—In consequence of the extent of oozing, Dr. Tanner sent for; patient a little delirious, but which soon passed away. The apparatus removed, and joint exposed, bleeding ceased, but shortly reappeared freely; joint again exposed; much startings in the limb; gets an opiate every four hours; slept for an hour or two during the night, and suffered little pain.

Tuesday, 27th. Pulse 100; tongue clean; pain diminished; starting ceased; bandages removed, and a many-tailed one substituted; the opiate continued.

28th. Pulse 110; tongue foul; was ordered a mild purgative and diuretic.

29th. Pulse 90; tongue much cleaner; leg required a little extension, a slight bow appearing in it; ordered an anodyne at bed time.

30th. Complains of much pain just above the pubes; has a slight suppression of urine; passed none for twenty-four hours; ordered a diuretic; better towards evening.

31st. Much improved; medicines continued.

March 1st. Improving; complains of no pain over pubes; wound examined; two inches of union have taken place, the remainder looking healthy.

2nd. Same as yesterday; wound looking healthy.

3rd. Little suppuration since operation, in comparison to the extent of wound. No stimulants given since operation, but a generous diet with broth.

4th. He steadily improved, and as I should say very fast, up to the time he left hospital, which was on the 22nd of June, 1860, four months from the date of his admission, three from the operation.

When leaving hospital, the limb was straight, very little shorter than the other; he could walk very well; suffered no pain, and his general health was very much improved.

Before concluding, I beg to draw your attention to one of the steps of the operation as performed by Surgeon Tanner. I allude to the manner in which the heads of the bones were removed,

which I do not recollect to have been done by any other person, and which is a great improvement to the operation.

It is recommended by authors, and one in particular (Surgeon Butcher, of Dublin, who has thrown great light on this subject), to use a particular kind of saw invented by himself. The qualities in it, he says, are, that it protects the soft parts and vessels, and where it is required to cut the bone obliquely. Now, in the operation as done by Surgeon Tanner, he used the plain amputating saw; and by placing the bones one on the other the vessels and soft parts were protected; and owing to the strength of the blade, he was able to cut straight without the blade bending, which I think might possibly occur with Surgeon Butcher's saw.

When the patient left hospital he returned to his home in the west of the county of Cork.

Since the above was written I have great pleasure in stating that the patient is alive and doing very well, and does not suffer in the least.

MARCH 27.

DR. BAINE in the Chair.

DR. SANDHAM exhibited the leg of a child, eleven days old, which he successfully amputated on account of a gangrenous ulcer above the ankle. He states: On the 11th of September, 1860, I was sent for in a hurry to attend Mrs. Margaret M——, aged 27. I found her much exhausted from hemorrhage, which introduced her labour. On examination, as was to be expected, I discovered a placental presentation, and the foetal presentation was also preternatural. I at once turned and delivered, without much difficulty; the secundines speedily followed, and the hemorrhage and urgent symptoms ceased immediately. When the nurse was washing the child, she discovered the phagedenic, or rather gangrenous ulcer you see in the preparation, situated above the internal malleolus of the right leg; the tarsal extremities of the tibia and fibula were dislocated from the astragalus, or rather their epiphyses were removed, and they projected out of the ulcer; the tendons were exposed, and a foul, offensively-smelling ulcer occupied the soft parts around. I ordered a yeast poultice, which was continued for ten days, and to nourish the child. Healthy inflammation set in, and natural amputation progressed. This encouraged me to propose to the parents its removal by amputation, and at the same time I assured them of the probability of the child's dying during the operation. On the eleventh day, the grandmother holding the child in her lap, and assisted by my son, I took off the limb below the knee by a flap amputation, in order to have as large a cushion as I could, knowing that, should the child survive, interstitial growth would still go on in the tibia and fibula. The child, very small, and a female, only winced during the tegumentary incision. She sucked sugar during

the rest of the operation. One vessel required a ligature. The operation was performed on Saturday, the 22nd of September; the stump healed kindly, without any untoward symptom, almost by the first intention; the child improved, took food freely, arrow-root and a little wine, and I discharged her well on the 4th of October, twelve days after the operation.

This woman received no injury during the period of gestation. She and the husband deny ever having had syphilis, or any other venereal complaint. Now, the question for discussion that arises in this case is of some importance, as I, for one, am taught by it to be very cautious in future in pronouncing such and analogous appearances on children after birth as positively secondary, tertiary, or quaternary syphilis. Well, then, the question arises—is the specimen of disease now before us, which commenced in utero some considerable time before delivery, the result of a syphilitic taint? and, if not, what is it, and how produced? As an argument in favour of its being syphilitic in its origin, the mother, who is very small, but hardy, had four children before this one; all came to the full time; two were still-born, and two survived birth but from two to four hours. On the other hand, the fact of the stump healing so very kindly, and the child improving, is, in my mind, very much against the idea of its being syphilitic—there was not another diseased speck on the child. I must candidly confess that the subject of the preparation before you is now a short time dead, but certainly from nothing as the result of the operation, or of a syphilitic nature. I infer, from the statement of the grandmother, that some lung affection carried her off.

An interesting discussion followed the reading of the case, and all were of opinion that the disease was not syphilitic.

Dr. Sandham here said—I was present at the trial of a young woman and her mother during the present assizes for infanticide, and Dr. Donovan, of Skibbereen, was medical witness for the prosecution. He stated that he had read in Dr. Collins' work on Midwifery that he (Dr. Collins) has often heard the cry or screech of an infant, not only during the passage of the head through the vagina, but even while absolutely within the uterus. This struck me at the time, and I am now anxious to get the opinion of all present with reference thereto. I have had some experience, but never heard a child cry or screech under the circumstance stated. None present ever heard such a cry, and all appeared to be of opinion that it was impossible for a child to cry so as to be heard, or cry at all, while the head was within the uterus, or without the uterus in the vagina.

*Subacute Inflammation of the Membranes and Substance of the Brain; Ramollissement.*—DR. FINN exhibited a pathological specimen of the above, and detailed the particulars:—

Sub-constable John Doyle, aged 23, unmarried, of robust figure, was received into hospital on the 12th March, labouring under irritability of stomach and pain of head. His illness, which



commenced about eighteen months before, was referred to over-excitement consequent on active interference for the purpose of quelling a riot. On the evening of the day on which the riot occurred, he was guilty of some excess, having partaken rather freely of punch. On that night he fell out of bed, and on the following day complained of vertigo. This symptom having continued for ~~some days~~, he was sent to hospital, where he was under treatment for six weeks. On leaving the hospital, he was able to resume his duties, which he continued to discharge to within a month before last admission. On being questioned, he complained of pain in the head and behind the ears, the pain increasing in severity towards evening and night, and interfering much with his rest. Pulse 88, small; costiveness; coated tongue; heat of skin; eyes prominent and staring, both pupils being much dilated; slight ptosis of the right eyelid, and the angle of the mouth at the same time was pendulous. It was also observed that the tongue, when protruded, deviated to left side. He suffered at irregular intervals from paroxysms of fever, attended with excessive secretion from the skin, and during which his consciousness was more or less disturbed, but never to such an extent as to preclude his replying coherently to questions addressed to him. Towards the close of life, his voice became gradually more inarticulate; he retained, however, to the last, control over the voluntary muscles. Death took place quite suddenly on the 26th March, as if from an apoplectic seizure.

*Autopsy.*—Congestion of the membranes of the brain, with sub-arachnoid effusion; generally increased vascularity of the brain; considerable amount of serous effusion in left ventricle, effusion in the right to a moderate extent; in the anterior part of the left middle lobe was discovered ramollissement to a moderate extent; in no other part was there observed any deviation from the normal consistence.

*Remarks on the treatment of Acute Disease.* By DR. CUMMINS.—I have been requested to re-open the very instructive discussion on the subject of acute inflammation, to which Dr. Belcher's paper gave rise; and with that view, rather than because they possess any features of peculiar interest, I beg to lay a condensed note of the following cases before you:—

Mr. S., aged 24, tall, thin, and sallow, of a delicate strumous constitution, has had a cough since he was a boy, when he suffered from inflammation of the lungs.

I was called to him at 5 A. M., of the 2nd of October, 1860, and was informed that he had suffered from rigors, headach, weariness of back and limbs, great thirst and prostration; has not slept for two nights; pulse was very rapid; tongue coated behind, dry and red at tip and edges. I prescribed a foot-bath, and minute doses of tartar emetic, &c., &c. This treatment was continued during the day with some relief, and at bed-time he took aperient pills, and a

draught the following morning; up to this time there was no cough or pain in the chest.

October 3rd. Good night; headach gone; but there is a short troublesome cough; inability to lie on the left side, where there is also a dull aching pain; pulse 108, weak, intermittent and compressible; respirations 24; expectoration copious, viscid and rusty; tongue moist, and covered with yellow fur.

*Physical signs.*—Comparative dullness over base of left lung, without vocal resonance or bronchial breathing, fine crepitus much marked over entire base; dry and moist bronchial rales over both lungs anteriorly and posteriorly.

On account of the state of the pulse, it was not without great misgivings that I ordered him a quarter of a grain of tartar-emetic every three hours. I also ordered a blister to the left side.

I visited him at mid-day, and found that the pulse had become firmer and more regular under the influence of the medicine.

October 4th. Pulse 100, regular; respirations, 28; pain less; expectoration as before; tartarized antimony, half a grain every third hour; beef-tea.

5th. Pain gone; can lie a little on left side; pulse 108; respiration, 28; complete dullness of left base, with bronchial breathing and bronchophony. Another blister to side; tartarized antimony, one grain every third hour; beef-tea.

6th. Improving; continue medicine, take 4 grains of blue pill, and 1 grain of powdered opium at bed-time. The medicine has not once caused vomiting.

7th. Spent an excellent night, and is much better; pulse 94, regular and soft; respirations 24; continue medicine and pill as before.

8th. Pulse 76; respiration 22; sputa diminishing, and is now frothy, and free from coloration. There is still considerable dullness over base of left lung, with marked bronchial breathing, and bronchophony, some crepitus surrounding dull portion. Nose bled freely. Omit tartar-emetic. Take three grains and a half of blue pill with Dover's powder, every four hours. Appetite for the beef-tea and toast increasing.

9th. Pills caused some vomiting. Pulse 76; respiration 24. Continue pills. In the evening respirations were down to 20. Continue pills. Wine 3oz.

10th. Improving; but cough is rather troublesome. Wine 4oz. daily.

11th. There is still considerable dullness, with bronchial breathing and voice, but no surrounding crepitus. Nose bled a little again. Respiration 23; pulse 76. Gums slightly affected. Continue pills twice a day. Take a mixture of ipecacuanha, ammoniacum, &c., when cough is troublesome.

13th. Convalescing rapidly. Continue pills at bed-time, and mixture as before. Expectoration now muco-purulent.

15th. It is astonishing how rapidly the solid portion of lung is resolving, and that without crepitus redux; can lie now perfectly well on either side; expectoration is diminishing; pulse and respiration are natural; takes an egg for breakfast, and meat for dinner, with good appetite, to which I add porter to-day. Take one grain of quina three times a day.

24th. I examined the chest carefully to-day, and find the dullness, bronchial breathing, &c., quite gone; respiration natural everywhere; cough gone; the acute attack seems to have carried off with it the old chronic cough to which he was subject. (Note—This did not return again).

Master E., aged three, had a most severe attack of croup about six months ago in England, and narrowly escaped with his life.

At one, A.M., on the 23d of August, 1860, I was called to him, and informed that, without any premonitory symptoms, he was again attacked with croup last evening.

I found him gasping for breath, with the most marked and severe tracheal respiration, and cough; face almost livid; skin hot; pulse very quick and hard. It was one of the most severe cases of the disease I had ever witnessed.

I immediately had him put into a warm bath, had hot fomentations applied to his neck, and gave him one-eighth of a grain of tartar-emetic every five minutes until he had taken a grain; after which he vomited, cough became soft, respiration became comparatively free, skin soft, pulse less frequent, and I left him about three o'clock out of danger. I directed that the medicine should be continued every three hours, and next morning lengthened the intervals to four hours; at that time I also gave him three powders, with a grain of calomel in each, which he took during the day. In two days from the commencement he was perfectly well. I could bring forward many cases of pneumonia and croup treated in the same or a similar manner, with the same satisfactory result; and I cannot call to mind a single fatal case of pneumonia, except an *old* man, many years ago, of the name of Millar, and I entirely forget how he was treated. Now, when it is remembered that I have been a dispensary physician for the last seven years, and that pneumonia is rather a common disease among the poor, I find it difficult to account for the difference between my results and the statistics of "tartar-emetic treatment," as given by Dr. Bennet, from Rasori (viz., one death in four and a half cases), except on the principle that I rarely exceed one grain every three hours, and omit the medicine as soon as there is evidence of solidification to any great extent.

I quite agree with those gentlemen who believe that inflammation must run a definite course when once established, and that venesection or even excessive local depletion may so reduce the vital powers as to render them incapable of perfecting the organization of the exudation which is thrown out in the second stage, thereby causing tedious convalescence, or still worse, its death (gangrene);

but I do not wish to ignore altogether the first stage of acute diseases, and to make the essence of inflammation to consist in exudation, as some seem to me to do. I believe that there is a stage of increased vital activity, and that the higher this runs, the greater will be the exudation which must follow. I believe that this first stage is more or less under our control, and that we should moderate it by tartar-emetic, local depletion, &c., never forgetting that a time must come when the safety of the patient will depend upon an active circulation in the affected part. It is because I think the lowering effect of tartar-emetic is temporary, and ceases a few hours at most after its use is suspended, that I value it more highly than any means that directly remove blood from the system—blood which cannot be replaced except by nutrition, at a time when the assimilative powers are more or less in abeyance.

I have no doubt many cases of pneumonia would recover if left to nature entirely; because, important as the lung is, a portion of its tissue may be incapable of exercising its function for some time, without much evil resulting; but is this the case in croup? Can we in such a disease sit down patiently and watch nature organizing the exudation, and absorbing it after its transformation, and the excretory organs beautifully casting the effete matter from the system? No; because we know that the exudation here, by preventing the access of air to the lungs, would certainly destroy life before nature had time for her operations. Surely, the most ardent and devout worshipper at the shrine of nature would attempt to anticipate, or at least moderate, exudation in such a disease: and, if able to do it in this, why not attempt it also in other acute diseases? All Dr. Belcher's cases came under treatment in the second stage, when exudation had already solidified the lung; in such cases I have long given up the use of calomel and opium, as not only useless, but positively injurious; contenting myself with a mild alterative course of blue pill or hydrargyrum cum cretâ, and even that rarely pushed even to plight ptyalism, never to salivation. Dr. William Townsend's cases have, however, shown that even the mildest preparations of mercury are unnecessary in the treatment of pneumonia, and if unnecessary, they are certainly more or less injurious. I am determined to put a complete non-mercurial plan to the test; and if my cases turn out as well as his have done, I shall be fully satisfied.

I should remark that the case of pneumonia I have brought forward occurred in a peculiarly unfavourable subject, and that it was complicated with general bronchitis, as indicated by the sputa, as well as by the physical signs.

*A Case of Fracture of the Femur, treated with Butcher's Splint.* By THOMAS POWELL, L. R. C. S. I., Medical Attendant, Dispensary, Enniskeen, late Assistant-Surgeon, H. M.'s. Bombay Army; formerly Lecturer on Practical Anatomy in the Peter-street (now Ledwich) School of Medicine, and Resident Surgeon, Anglesey Lying-in-Hospital, Dublin.

On the 14th of August, 1860, I saw W. L., a well-made, muscular, dark-complexioned man, five feet eight inches high, and 37 years old. On the night before, when returning from a fair, he had fallen on the road, and found himself unable to rise; a neighbour, passing by soon after, put him in a cart, and brought him home. When I saw him, he was lying on the floor, apparently in great agony. There was one man pulling the foot, and another pressing the thigh between his open hands, which the patient said relieved him. On examination, I found the left leg  $1\frac{1}{2}$  in. short, the foot everted, and the thigh flattened and thickened. Upon making extension, crepitus was easily obtained; and I determined the existence of fracture of the femur in its middle-third. I immediately prepared splint and bandages; and, after bringing the limb to its normal length, I applied the long splint in the manner practised and with the addition recommended by Mr. Butcher, of Mercer's Hospital. The man immediately expressed himself as much relieved; on measurement, the limbs were the same length; and the startings of the muscles, which had caused great pain, subsided. Before I left him, he expressed himself as free from pain, and the altered expression of his countenance testified to the fact.

August 15th. Free from pain. Passed the night well.

August 29th. The length of the limb is still retained; free from pain and irritation of the skin, Reapplied the retentive bandage to the foot, and changed the perineal band,

September 11th. Changed the perineal band.

October 1st. Removed the apparatus, and applied a starch bandage from the knee to the perineum. The limbs are the same length; the foot holds its proper direction; the knee is a little stiff.

October 8th. Removed the starch bandages; the man moves about on crutches; the knee still stiff.

October 17th. Motion freer in the knee; he goes about on one crutch most actively.

October 31st. He this day discarded the crutches; when going out of doors, he carries a stick, but moves about the house without any assistance.

This case serves to illustrate in a remarkable degree the advantages of the practice introduced by Mr. Butcher, occurring in a distant part of the country, where the daily attention of the medical attendant was impossible. By reason of its superior power of retention, and the physical ease and in consequence endurance which the patient obtained, it enabled the fragments to be kept in a state

of perfect coaptation; and the case, under most unfavourable circumstances, brought to a successful termination. The superiority of Butcher's splint has been proved in many hands; but as this case shows such admirable results after so rough a test as country practice is to retentive apparatus, I think it tends to strengthen the evidence in its favour and adoption.

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*Case of Poisoning with Camphor.* By THOMAS WOODS, M. D., Parsonstown.

Mrs. P——, housekeeper, aged about 30, on Easter Sunday morning, intending to take a wineglassful of camphor mixture with a little sal volatile, for headache, took instead, by mistake, a wineglassful of a solution of camphor in alcohol. There were  $2\frac{1}{2}$  ounces of camphor in 8 ounces of the spirit; she, therefore, must have taken 330 grains of camphor, with about a teaspoonful of sal volatile. She instantly felt a burning sensation in the mouth and stomach, and called for water. When the messenger returned, she was insensible, and vomited about 6 ounces of "something like her breakfast," according to the servant's account.

I saw her in about fifteen minutes after the taking of the camphor. She was quite insensible, but not comatose. She was rolling her head from side to side, and was very uneasy; her skin was slightly raised in temperature, and her pulse was full, but not much quickened; her breathing was not hurried, and the pupil of the eye was natural. She could not be got to swallow anything, and in about twenty minutes she had an epileptic fit of great severity, which lasted for about five minutes. She became very livid, almost black, in the face, and foamed slightly from the mouth. I endeavoured, when the fit was over, to introduce the tube of the stomach-pump, but could not succeed, owing to the obstinate resistance unconsciously made by the patient whenever it was got into the mouth. Even this was a matter of much difficulty, from the tossing about of the head, and rigid closing of the teeth.

I took away twelve ounces of blood, and placed in the mouth tartar emetic and ipecacuanha wine in rather large doses, but with very little effect, as vomiting was hardly induced, and I saw no camphor in what was evacuated. I also had the bowels well cleared out with enemas of warm water, and cold water dashed on the face and neck. These means were all that were used; and she regained consciousness in about an hour, and gradually recovered, being in twelve hours free from all bad symptoms, complaining only of weakness and slight nausea.

The interest of this case lies, not so much in the quantity of camphor taken (although I believe it is the largest on record) because I do not know how much might have been thrown up before I saw her, but in the well-marked epileptic fit, which occurred in about half an hour after the dose was taken; and in the absence of all

symptoms of after-irritation of the stomach, or affection of the head or heart, although a sufficient quantity of the drug must have remained in the system to produce the immediate symptoms related. Since the above was written, I have heard that for about a week after the dose was taken the intellect was not quite so clear as usual, especially after sleep. The patient fancied every day was Sunday for half an hour after rising from bed. There was induced also an unusual torpidity of the bowels.

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*On the Therapeutic Influence of the Southern Climatic Sanatoria, particularly with reference to Chronic Tuberculosis of the Lungs.* By Dr. RULLMANN, of Wiesbaden.\* Translated from a reprint from the *Würzburger Medicinische Zeitschrift*, 1861. Band 2. By WILLIAM DANIEL MOORE, M.D., T.C.D., M.R.I.A.; Honorary Member of the Swedish and Norwegian Medical Societies.

It is an ancient medical practice to send chronic patients, and especially those affected with pulmonary disease, to seek for cure or amelioration in a southern climate, and it has also long been a public custom to travel for health's sake to the south; yet until lately this has been accomplished only by a few. But since Europe has been intersected in almost every direction by railways, since the iron road has surmounted the very Alps, since steamships traverse the Mediterranean Sea in regular postal routes, enabling patients without danger rapidly and easily to take an extensive journey towards the south, hundreds of invalids wander annually, on the approach of winter, from the northern regions of Europe, to look for recovery in a more southern climate. It is chiefly the coasts of the Mediterranean which are thus resorted to by patients. Gradually certain places have come into repute, which, on account of their favourable local and social circumstances, are regarded as the most suitable for invalid stations, and which are now designated as southern climatic sanatoria. Of these the following should be placed in the first rank: Malaga, Hyères, Nice, Venice, Pisa, Rome, Palermo, Cairo, Algiers, Madeira. These are the places at present most visited and recommended, and with which we are medically best acquainted. In the second series those may be enumerated whose importance as climatic sanatoria is as yet but little known, has only recently been suggested, or is by many altogether doubted; such are: Cannes, Villafranca, Mentone, San Remo, Naples, Messina, Catania. The climatic sanatoria of the Alpine regions, as Meran, Gries, can no longer be considered in like manner as southern sanatoria.

Though a southern climate has long been esteemed and used in

\* Read in abstract at the Congress of Physicians of the Middle Rhine, at Frankfurt, on the 18th of October, 1860.

medicine as a remedy, and although the southern climatic sanatoria have for many decennial periods been visited by a great number of patients, the state of medical knowledge as to these localities is still very defective, and special indications for the several places can scarcely be laid down. If we compare the knowledge we have with respect to them with that which we possess as to our medicinal springs and baths, we shall find that we know incomparably more and much more certainly about the latter, although even with respect to them medical indications are still very vague and undefined. While we have accurate physical and chemical investigations of the waters of our bathing places, we are unsupplied with the necessary accurate observations, continued through many years, of the meteorological conditions and other cosmical agents of our southern climatic sanatoria. With regard to baths, we find the results of much medical experience upon record. But we shall in vain seek in medical literature for frequent and accurate reports on the effects of a southern residence in various diseases. A well-founded scientific treatment of this subject has only most recently been commenced; and only quite lately have good monographs been published on some southern climatic sanatoria, as that of Mittermeier on Madeira (1855), that of Reil on Egypt (1859), that of Vivenot on Palermo (1860), that of Sigmund, &c. But much still remains to be done. These very writers complain loudly of the defective and uncertain climatology of these places and of the sparing medical material which still exists. Consequently, there prevails among medical men in general a great uncertainty in the appreciation and use of the southern sanatoria. Practitioners are often in doubt as to what disease is best adapted to a southern climate. On the one hand, too much is hoped for—on the other, too little is expected, from this change of climate. Indeed, the beneficial influence of a southern climate on the disease against which this has hitherto been most employed, namely, chronic pulmonary tuberculosis, has been altogether questioned. But, notwithstanding this uncertainty and diversity of opinion, resort to southern climatic sanatoria yearly increases; and a more accurate study thereof, and the establishment of definite indications, constantly become more important and more urgent. In the following pages I shall endeavour to fix the points of view on which the study of these localities must specially be based, and to propound the principles suitable for the estimation of the medical material at present accessible, and I should hope thereby to contribute something to the due appreciation of these sanatoria.

In my opinion it is advisable in the first instance to consider only those diseases in which a southern climate has hitherto been most recommended and employed remedially, and with respect to which, consequently, the largest amount of medical experience is on record. These are chronic bronchial catarrh and chronic pulmonary tuberculosis. Let us first inquire: is amelioration or cure to be expected in these two morbid conditions from a residence in the so-called southern climatic sanatoria, have we scientific grounds for such a



belief, and does sufficient and reliable experience exist upon the subject? By such a limitation of the question, we shall sooner and more easily arrive at a sure and practical result. By endeavouring to investigate the several climatic agents and their physiological effects, we shall certainly obtain scientific points of support for the estimation of the influence of a climate on certain diseases; but these effects are as yet in general but little known; and even if they were generally known, we could not thence alone deduce the medical importance of the southern climatic sanatoria; because our knowledge of the climatic circumstances of these places is as yet very defective; besides, the physiological effects of given remedies admit of only a limited conclusion as to their therapeutic value: finally, we have here to do with a co-operation, the result of the sum of a group of very different cosmical and other influences. We are therefore compelled, in investigating these sanatoria, to invoke also the aid of empirical experience; indeed, as the matter at present stands, medical empiricism must still serve as the principal ground of our decision.

In the estimation of climatic sanatoria, especially with reference to their employment in chronic pulmonary diseases, three climatic elements have at all times been taken into consideration, namely, the temperature, the degree of moisture, and the pressure of the atmosphere. Even if these do not constitute all the influences which affect a person labouring under pulmonary disease, who resorts to the south for the recovery of his health, and which have an important bearing upon his morbid condition, they possess the greatest importance in our inquiry. The general opinion is that a moist warm climate, with an uniform temperature, is the most suitable to patients labouring under chronic pulmonary disease, especially to such as are affected with tubercle. As to the importance of different atmospheric pressure, there is still certainly much diversity of opinion. Some will have their tubercular patients exposed to a slight atmospheric pressure, and they quote the high pressure of the southern climatic sanatoria to the prejudice of the latter. Others consider the climatic sanatoria on the shores of the Mediterranean Sea so suitable as a residence for such patients, precisely because the atmospheric pressure is there higher than in most regions of the northern inland parts of the Continent. Such generally propounded and exclusive propositions have at all times justly met with opposition, and have not much promoted our knowledge of the climatic sanatoria. It has, on the contrary, only impeded investigation as to the remedial efficacy of a climate, that people, leaning on some empirical observations and physiological hypotheses, have seized upon a single climatic element and considered this to be the only active or only important agent in the cure, supposing all other elements of climate and other local influences to be unessential, and unworthy of closer examination. I am of opinion that both physiological theory, and also and especially medical experience, are against the assumption of the exclusive importance and absolutely necessary existence of certain climatic factors in the cure of those diseases. Even if we must suppose that

the greater warmth of the southern winter is always favourable to our patients, who for chronic affections of the lungs visit the southern sanatoria, I do not believe that a great uniformity of temperature, or a high degree of hygrometric moisture, is absolutely necessary: indeed, in many cases the latter is not even desirable, and just as little does a particular amount of atmospheric pressure appear to me to be an essential condition to recovery. I rather believe that these meteorological elements may be present in different degrees and in various combinations, and yet—with the co-operation of various factors extrinsic to climate—may produce an equally favourable total effect.

All these climatic sanatoria possess in common a greater degree of warmth, and this is certainly to be regarded as an extremely important curative agent in chronic affections of the lung. That an elevated degree of atmospheric temperature exercises a beneficial influence on our pulmonary patients, and especially on those affected with pulmonary tubercle, is proved by the fact that these patients always feel better in our summer than in the cold seasons of the year—winter, the beginning of spring, and the end of autumn; as well as by the fact that individuals affected with chronic diseases of the lung, when transferred from the north to the south, recover and become comparatively well; while, on the other hand, those who migrate from the south to the north are very frequently and seriously visited with pulmonary diseases. Moreover, the warmth must not exceed a certain degree, if it is to work beneficially on our northern patients. In this respect I would call attention to the fact, that all southern climatic sanatoria, to the efficacy of which in chronic pulmonary diseases medical experience bears testimony, lie within the temperate zone. But the tropical zone, according to statistical investigations as to the disease and mortality of the French and English colonies, is extremely prejudicial to European immigrants who are predisposed to pulmonary tuberculosis, or may be in the early stage of that disease; and it is a well-ascertained fact that within the Tropics chronic tuberculosis of the lung very easily runs into acute and rapid pulmonary phthisis\*.

In Cayenne the French exiles die not merely of yellow fever, dysentery, and other southern diseases, but also in large numbers of pulmonary phthisis; and in Ceylon, Calcutta, and Madras, the same disease is said to find many victims among the English immigrants.

We cannot at all, or only under strict precautions, recommend to such patients a summer residence on the shores of the Mediterranean, even though remaining there during the winter may be highly advantageous to them. Indeed, we observe that these patients are worse even in our northern summer, when the latter is unusually hot; and it is, with reason, thought advisable to send such patients

\* See J. Rochard, "*De l'Influence de la Navigation, et des pays chauds sur la Marche de la Phthisie.*" Paris, 1856; "*Gazette Hebdomadaire,*" 1856 and 1857.

during the hot period of our summer to cool localities—to mountains, to the sea coast, or to sylvan districts. How far an elevation of temperature may in those cases be advantageous will, perhaps, be best measured by the temperature of an ordinary summer.

Much importance has been attached to uniformity of temperature, and it is certain that great and sudden changes of temperature act injuriously on those affected with pulmonary disease; but a moderate alternation has, for the most part, a beneficially exciting and strengthening influence on them, particularly on those who are of weakly constitution, while a great uniformity may produce a prejudicial relaxation. We observe this often in our summer. When an elevated temperature has prevailed during the day, a mild night and morning coolness has a peculiarly animating and refreshing effect, both on the healthy and on invalids. Alterations of temperature have no longer an injurious influence, when they occur with a certain constancy and regularity. Thus, for example, in the Egyptian winter the variations of temperature between early morning and noon are not inconsiderable, but as they occur to-day, they return on the following day in a similar manner\*.

As to atmospheric moisture, a certain degree of it has by many been considered as an essential condition of the utility of a southern climatic sanatorium in chronic pulmonary affections. It is, in fact, not to be disputed that a moist warm atmosphere is very frequently of decided advantage in chronic diseases of the lungs, by reason of its loosening and generally composing effect; and southern climatic sanatoria with considerable atmospheric moisture, as, for example, Madeira, may exhibit numerous favourable results—indeed, it may be said that the southern climatic sanatoria hitherto most resorted to and esteemed lie on the seacoast, and must, therefore, present a certain moisture of the air. But, on the other hand, it is not to be denied that in many instances a dry, moderately warm air, by its secretion-diminishing and general vivifying effects, and by promoting nutrition, may have a favourable influence on the course of those diseases; and there are medical observations on record, according to which a relatively dry warm climate has afforded special service as a remedial means. That in the adoption and estimation of the several southern climatic sanatoria the high degree of moisture is no longer in practice considered to be the principal point, is proved by the fact that of all the climatic sanatoria of the French coast physicians now give the preference to the drier Hyères; in like manner Algiers, which, in comparison to the other places on the shores of the Mediterranean, presents a very slight degree of atmospheric moisture, is preferred by many physicians to all the Italian and French climatic sanatoria. Moreover, the air of Egypt, of the suitability of which as a winter residence for patients labouring under chronic affections

\* See my Reports on the Climate of Egypt, "Deutsche Klinik," 1859, Monatsbl. 4; and "Archiv f. Phys. Heilkunde," 1859, iii. 389.

of the lungs we have the most positive proofs, is drier in winter than that of any other similar sanatorium; compare the meteorological observations of Coutellé, Destouches, Reyer, Uhle. Indeed, in Egypt patients are even recommended to make excursions to the desert, with its fine, moderately warm, but dry, air, and often to sojourn there; and it is in that country precisely the districts adjoining the desert, or easily accessible to its air, which are thought most suitable for permanent invalid residence. These are Cairo, to whose gates the desert extends, and the narrow valley of the Nile, above Cairo to Nubia, the so-called Upper Egypt, which is compressed between great desert plains\*. But the damp Delta, and the still damper sea coast, with Alexandria, are avoided by invalids. When we send patients affected with pulmonary tubercle in summer to high mountains, we can certainly not reckon upon much moisture in the atmosphere, and yet many such invalids are very well there. I would also observe that in countries whose climate is notoriously to be classed among the dry, pulmonary tuberculosis rarely occurs, as in Egypt and in the steppes of Kirghiz\*. All these statements suffice to show that of the several climatic elements which may influence the development and course of chronic pulmonary tuberculosis, dry air is not to be regarded as an absolutely unfavourable agent; but that, on the contrary, it may in many cases have a positively favourable effect.

The influence of the various degrees of moisture is, indeed, for the most part, exercised upon the lungs and the system at large. Thereupon there exist no accurate physiological experiments. But they possess, moreover, an indirect influence, inasmuch as the moisture modifies the temperature and its effect upon the organism. Thus it may be taken as a rule that the moister the climate is, the more uniform will be the temperature; but it is at the same time to be observed that the moister the air, so much the more will the cold be felt, particularly when the air is in motion, and so much the more penetrating will be the variations of temperature; while, when the air is dry, neither the cold nor the variations of temperature, which in this case are certainly more considerable, are similarly felt. By the humidity of the air I mean the amount of watery vapour appreciable by hygrometric measurements; and it is, there-

\* It is an error to suppose that the patients frequenting the Nile, in Upper Egypt, are, by constantly remaining on ship-board, always exposed to a damp atmosphere, and that thus the beneficial influence of the climate upon them is to be explained. The air in Upper Egypt is to be considered dry, not merely on the shores of the Nile, but also on the river itself. Uhle, who made all his hygrometric observations on board-ship, states that "the air on the Nile between 24° and 22° of Northern latitude is one of the driest of those as yet examined." See Uhle, "Der Winter in Oberägypten."

† Compare the writings of Pruner, Griesinger, and Reyer on the Diseases of Egypt, and Dr. Neftel's "Beobachtungen aus den Kirgisen-Steppen. Würzburger Med. Zeitschrift," 1860.

fore, to be distinguished from showers and rain, which may likewise have an important influence upon disease. Thus the rarity and short duration of rain is to be considered as a favourable element, as the patient is thereby afforded the opportunity of frequently enjoying the open air, and taking suitable exercise. It is also to be observed that the humidity of the air, that is the amount of watery vapour contained in it, does not directly coincide with the frequency of rain. It is indeed true that with a dry climate a greater rarity of rain is connected; and also that, where rain frequently falls, a greater amount of moisture can be demonstrated in the air; but frequency of rain is not necessarily connected with a moist climate. Thus, for example, Venice has in winter, according to hygrometric measurements, the dampest air of all known southern climatic sanatoria, and still less rain falls there in the winter months than in any other Italian or French climatic sanatorium (except Nice), and less than in Madeira.

As to the influence of high and low atmospheric pressure upon the development and course of pulmonary tuberculosis, directly opposite opinions have been advanced. On the one hand it has been asserted that a high atmospheric pressure acts most favourably on the course of this disease, and on the other it has been stated that a low pressure is to be regarded as a favourable element in the treatment. In support of each opinion, physiological reasons and empirical proofs have been brought forward. On the one side it has been advanced that all southern climatic sanatoria, of which we possess favourable experience, lie on the flat sea coast, and are consequently exposed to a high degree of atmospheric pressure; that the physiological effects of even strongly compressed air, as in the occupation of diving and in the works connected with the building of new bridges, are in general well borne; that the physiological changes produced by high atmospheric pressure, as retardation of the respiration and circulation, driving back the blood from the peripheric parts, development of the so-called venosity, can act only advantageously on the course of those diseases; and that, finally, direct experiments have established the favourable influence of compressed air upon chronic pulmonary diseases, and especially upon chronic tuberculosis of the lungs\*. On the other hand, it is stated that experience shows that many patients labouring under pulmonary tubercle are very well in high situations, and therefore under diminished atmospheric pressure, and that they have been frequently sent with advantage to lofty mountains (Righi, Gais); that, moreover, the physiological effects of diminished atmospheric pressure, as increase of appetite, improve-

\* On the therapeutic employment of compressed air in the so-called baths of compressed air of Pravaz, Milliet, Devay, see "Canstatt's Jahresbericht," 1854, ii. 226, and "Gazette Hebdomadaire," 1859, 783. Further, an essay on the Influence of Altered Atmospheric Pressure upon the Human Organism, by Vivenot, "Virchow's Archiv," xix., 492.

ment of digestion and of blood-formation, acceleration of the circulation, and promotion of the metamorphosis of tissue\*, must be advantageous to patients with weakened constitutions; and, lastly, it is alleged that pulmonary tuberculosis diminishes in frequency with the elevation above the level of the sea<sup>b</sup>. But even if these physiological and empirical statements which have been brought forward on either side in support of the different views of the writers are in themselves correct, they can by no means prove the general and exclusive validity of any of these opinions. It is much more probable that, in these pulmonary diseases, there exist different conditions and stages, as well as diversities in the local affection and in the general health, according to which at one time a diminished, at another an augmented, atmospheric pressure will be advantageous. What the extent of this advantage is, or how great is the physiological influence of the varied atmospheric pressure, as it occurs at the ordinary elevations and depressions of the surface of the earth, is not to be decided by our present experience; but it is certain that in many cases the climatic influence, as well as agents extrinsic to climate, act so powerfully upon the organism, that the atmospheric pressure is not to be considered, its influence being masked or counteracted. Lastly, it may be mentioned that the effect of atmospheric pressure, even if it is considerable, does not depend solely on the absolute elevation, but also—apart from the morbid condition—on individuality and habit, as the action of the same atmospheric pressure must be different, accordingly as the patients have previously lived under another more or less different pressure.

For a climatic sanatorium, the purity of the air is also of great

\* Dr. Brehmer, who is a zealous advocate of the opinion that lessened atmospheric pressure is the most important agent in the cure of pulmonary tuberculosis, includes, among its physiological effects, diminution of the determination of blood to the lungs, and he appeals in support of this view to experiments by Volkmann and Poiseuille? "*Baln. Zeitung*," viii. 289.

<sup>b</sup> [Dr. Mübry, for example, in his work on Climate, "*Klimatologische Untersuchungen*," &c., Leipzig und Heidelberg, 1858, attaches the greatest importance to the amount of atmospheric pressure to which phthisical patients are subjected. He states that "the occurrence of phthisis diminishes with the atmospheric pressure in vertical elevations;" and he suggests that sanatoria almost ensuring an immunity from phthisis, "might be found on the eastern side of the Andes, in Mexico, or in the Sierra of the Andes of South America." See *British and Foreign Medico-Chirurgical Review*, vol. xxiii., p. 62, where I have contributed an analysis of his work.—TRANSLATOR.]

\* The physiological phenomena usually ascribed to changes of atmospheric pressure, may as well be attributed to other atmospheric changes present at the same time,—alterations in temperature, moisture, and current of air. Richerand says:—"Were it conceivable that—the temperature, the current of air, and the degree of hygrometric moisture remaining the same—there should be such a change in the atmospheric pressure as corresponds to the daily and yearly variations in the barometer, the effect of the latter upon the human system would be, not to say none, at all events so slight, that neither would it be subjectively felt even by the most sensitive persons, nor, *a fortiori*, would it be objectively demonstrable.—See Vivenot's Essay in "*Virchow's Archiv*" already referred to.

importance, and must certainly be reckoned among the favourable curative agents in pulmonary diseases. The air must be free from foreign organic and inorganic admixtures, especially from products of decomposition derived from the animal and vegetable kingdoms, from so-called miasmata, as well as also from irritating hard particles of dust. Now, we find the air particularly pure when it is frequently moved, when it blows over large, unbuilt upon, and not overgrown, surfaces, as well as in its higher layers, on which account a peculiarly pure air is possessed by those southern climatic sanatoria which are situated on isolated islands, as Funchal, in Madeira—on the sea coast, as the sanatoria on the shores of the Mediterranean Sea—or in the neighbourhood of the desert, as Cairo\*.

In a climatic sanatorium the winds come next under consideration, in so far as, from their origin and direction, as well as the intensity and frequency of their occurrence, they affect the temperature, the degree of moisture, and the purity of the air; and, under the influence of the winds, the action of these climatic factors on the system varies. Thus a moist and cool temperature acts much more intensely when the air is in motion than when it is at rest; on the other hand, winds moderate the effects of elevated and dry warmth. But strong winds may, in themselves, be important to those labouring under pulmonary complaints, as they mechanically impede and disturb the respiratory movements. In some places the winds require special attention, as their influence is so prominent as to give to a locality its climatic character, and thus to decide the suitability of such a sanatorium. Thus the maestral, a dreaded north-north-west wind, in the south of France, extends its effects to Nice, and renders almost the whole of the French littoral, except a few points protected by mountains, an unsuitable residence for pulmonary patients, especially those affected with tubercle, who are in an advanced stage of the disease. And those hot winds blowing from the interior of Africa, the Chamsin of Egypt, the Samum of Algiers, the Sirocco of Italy, which are particularly felt in the north of Africa, are said to determine patients, at the time of their occurrence, in April and May, to leave the south.

A climatic agent still remains to be mentioned, to which little value is usually attached, but the importance of which, to invalids, should not be underrated—that is, the clearness of the sky. "The degree of habitual clearness and brightness of the heavens," says Humboldt (*Cosmos*), "is important, not merely for the increased

\* It is incorrect to suppose that the air of the desert or its vicinity must necessarily be full of dust. The air of the desert, surrounding the valley of the Nile, is almost always extremely pure and clear. The sand of the desert is raised only by violent storms, almost exclusively by the storms from the south, which occur periodically in May. The dust met with at other times in Egypt is derived from unpaved clayey roads and places in the towns, the roads, and causeways at the Nile. In the plain and on the river itself, as well as in the adjoining desert, there is never, or only extremely rarely, any dust.

warming of the ground, the organic development of plants, and the ripening of fruits, but also for the sensations and entire disposition of man." When we consider that a cloudy day depresses a healthy man, while, on the contrary, a clear sky excites and enlivens his spirits, how much more is this not the case with an invalid; and how important, in this instance, is the reflexion thereof upon the bodily condition. But an invalid who leaves his home on account of his health, and lives in a strange neighbourhood and under unusual circumstances, has a very peculiar need of such external stimulus. A blue sky and bright sunshine give an additional charm to nature, and excite the beholder to more attentive contemplation; but the contemplation of nature occupies, calms, and rejoices the patient. His thoughts are withdrawn from his own sufferings; he is animated to the enjoyment of the open air, and to bodily exercise. In this respect we must regard the greater clearness of the sky, which is peculiar to southern regions, as in itself a circumstance favourable to the cure of those diseases.

There are still other climatic influences to be considered, which, indeed, are not usually of equal importance, but which, under certain circumstances, may have a very important bearing on the course of diseases. Such influences spring from the altered local and social conditions in which the patient is placed, from his altered occupation and mode of life. It is, therefore, important to be aware of, and to take into account, the particulars of residence, food, water, convenience, social pleasures, the opportunity of active and passive exercise, such as walking, riding, driving, &c.; also the mental enjoyment offered by the locality. Thence we derive, not merely direct influences on the physical condition, but also equally important influences on the intellectual life of the patient. If it be true that lasting mental emotions and excessive intellectual labour may have an unfavourable influence on the development and course of the diseases mentioned, the removal of such injurious agents may also have a favourable effect. How many of those patients, who have visited such southern climatic sanatoria, have been indebted for their improvement and comparative cure, perhaps, chiefly to the circumstance that, by the journey and change of scene, they have been withdrawn from injurious mental influences or excessive intellectual exertions. However, the new circumstances do not always act favourably. If many patients are agreeably stimulated and occupied thereby, others become dispirited and depressed. It is, indeed, in general, a great advantage when the patient finds in his sanatorium entertainment and diversion—and whether this is the case depends on personal inclination, on the degree of education, on the professional occupations and other individual circumstances of the patient; but it is also possible that this entertainment and diversion may be injurious. For example, Rome is recommended as a climatic sanatorium, partly on account of its treasures of art, which may afford to the invalid agreeable intellectual occupation and diversion; but it must not be forgotten that patients may



easily forsake the proper object of their visit, and make the study of the treasures of art the first point. In that case this occupation leads only to an injurious excitement; and the churches and halls, in which these treasures are located, are then to the patient nothing but frigid retreats, wherein he exposes himself to cold. In like manner, the patient is agreeably and suitably occupied in Egypt in considering the glorious antiquities on the banks of the Nile, if he does not in the study subject himself to undue exertion—but how easily does not zeal grow with contemplation! he drives, rides, goes from place to place, and so gives no rest to either mind or body. Nevertheless, the great utility in general of such intellectual influences is not to be denied. The agreeable intellectual impressions, the recreation and amusement of the mind, the light and desirable mental occupation connected with the journey, the novel residence, and the new life, may, in many cases, act even more powerfully on the patient and the course of his disease than the climate itself, and may compensate for, and render harmless, some injurious influences of the latter.

It would, of course, be very desirable, in the examination of the influence of southern climates on diseases, to have at our command a large mass of empirical information. When we review the records of medical experience with respect to the result of a residence in the so-called southern climatic sanatoria, we certainly find such in reference to chronic bronchial catarrh and chronic pulmonary tuberculosis. But these records proceed from very different sources, and are in part very vague and inexact. Here, too, therefore, careful criticism is required; and we must first ascertain what records are in general available, and how far they justify us in coming to a conclusion as to the therapeutic importance of a southern climate.

The value of observations as to the curative effect of a southern climate in chronic bronchial catarrh and chronic pulmonary tuberculosis has hitherto not been highly estimated, because these two diseases are so frequently confounded, the differential diagnosis is so seldom accurately established, and, in fact, in many stages of these diseases, and especially in those in which a curative effect might most reasonably be expected, it is not to be made with certainty. With respect to the cases on record of cured tuberculosis of the lung, we very frequently hear a doubt expressed, whether the disease was really pulmonary tubercle. But if we take into account only the instances where the differential diagnosis was established beyond all doubt, and include only the cases of pulmonary tuberculosis where ulcerative destruction of the tissue of the lung was objectively demonstrated, or the signs of general phthisis were distinctly manifest, not only would the whole mass of evidence become very scanty, but this would comprehend only those cases, where, from the advanced stage of the disease, a cure was scarcely to be expected, and where any journey and change of place must be a matter of serious consideration. I do not look upon the possibility of confounding the two diseases as of so much consequence in its bearing

upon the subject under consideration, and I believe that even those cases where the differential diagnosis is not fully established, nevertheless supply an available material, because the treatment of both affections is in so many respects the same. We have, in the treatment of chronic tuberculosis, often no other object than the accompanying bronchial catarrh; and if we have been able to allay, relieve, or prevent this, we have done much. Therefore most remedies employed in chronic bronchial catarrh are likewise recommended in chronic pulmonary tuberculosis; and we might infer that when a patient labouring under chronic bronchial catarrh is well in a given climate, this climate will also be favourable to a patient affected with pulmonary tuberculosis. In my opinion, the improvement of the general state is much more important in reference to the availability of observations, than the placing of the diagnosis beyond a doubt; and a local diagnosis is certainly sufficient to enable us with probability to infer the presence of pulmonary tuberculosis, or of a mere simple catarrh.

It would further be desirable that we had at command a certain number of cases to supply us, to some extent, with a guarantee that the "*post hoc*" coincides with the "*propter hoc*." I know very well that the greater number in themselves do not prove much, and a well-observed case is worth more than a hundred superficially recorded. But, nevertheless, a greater number of medical observations is here very much to be desired; for, when a patient leaves his home, to seek, in a strange land and in another climate, for restoration to health, he is liable to so many and various influences, other than climatic, which may contribute a favourable or unfavourable effect, that it is often difficult to decide what share the climate has had in the result. Frequent observations are necessary to enable us to say that patients, under the several circumstances and accessory influences in a certain climate, have reaped decided benefit, and that the climate has been the principal means of procuring the same.

If we now ask whether numerous and reliable records exist as to the curative effect of a southern climate, we shall obtain no very satisfactory answer. Although hundreds of invalids yearly travel to the south in order to regain their health, we possess only few exact and reliable medical observations on the subject; on the other hand, a number of opinions have been formed, which have, however, been based upon an unscientific application of experience. Many patients travel to the south without having consulted a medical man. Medical men, also, have often neglected to establish the diagnosis and the necessity for change of climate, and have rather yielded to the patient's inclination or his longing for recovery. If such patients then return cured or worse, or die at the place of their resort, where they expected health, premature conclusions are very frequently drawn from such results to the general efficacy of a climate. How often do not patients undertake a long journey to the south in a condition in which any journey is a risk. Of this I witnessed a striking example. In the winter of 1856-57, an English invalid

came to Egypt, who died of exhaustion in the railway train on his journey from Alexandria to Cairo, the place of his destination. A striking recovery, or an unfavourable result, if it acquire importance from the person to whom it occurs (it is sufficient to refer to the visit of the Empress of Russia to Nice, and of the French actress, Rachel, to Egypt), has often brought a climatic sanatorium into good or bad repute, while, perhaps, hundreds of other cases could have shown the unfounded nature of such credit or discredit, though, because they did not accidentally excite some general interest, they remained unknown or unattended to. Such unwarranted opinions have found acceptance both with the public and with physicians, but only very few certain medical observations have been made known.

With respect to the utility of the several empirical observations, it must also be considered how such a residence should be carried out. On this point mistakes are often made with and without the fault of the patients. Many invalids travel to the south, but do not procure, or fail to attend to, the necessary instructions for their new life, live unsuitably with regard to food, clothing, dwelling; or adopt an unhealthy occupation. Many do not wish to have travelled to Italy or Egypt without having seen the far-famed historical memorials and art treasures of these countries, and thus are led to use too great bodily exertion, and to travel from place to place, while their mind is kept in a state of constant agitation and excitement. In such patients the effect of a southern residence can scarcely be favourable, though it can prove nothing against the beneficial influence of the climate.

Of the injudicious use of a southern climate, the following case, furnished by my own experience, may serve as an example:—In Cairo I met with a North American, who came thither in January, 1858, labouring under pulmonary disease, and in a miserable condition. The first part of the winter he had spent in Italy, had travelled much about there, and in that short period had visited the principal cities of that country. In Cairo I lived with him in a villa close to the Nile, a league from the city properly so called. The journey to the city he very often made on an ass, which appeared to me to be a very great exertion for him; but he made still greater and more fatiguing tours. Although I never subjected this patient to a strict medical examination, and only occasionally had social intercourse with him, I was convinced, by the most striking signs, that he laboured under pulmonary phthisis, as, together with excessive general emaciation and weakness, he had a violent cough, copious viscid expectoration, often mixed with blood, and at the same time complained of feverish symptoms, sleeplessness, and dyspnœa. Besides the bodily exertions which he exacted from himself, I must mention the unsuitable diet he adopted; he drank strong wine, often brandy-and-water, ate, fruit, &c. In the following April I met him again on the voyage from Alexandria to Malta; he complained then, with good reason, that his residence in the south had

been of no use to him; he was in such a wretched state, that he had to be carried up and down the stairs in the steamer. In this instance the unfavourable course of the disease is certainly to be ascribed only to the injudicious use of the climate, and not to the climate itself.

Another striking example I met with in a young Hungarian. This patient had been sent to Cairo on account of pulmonary disease, and had there spent a year and a half. During his residence in Cairo he had made frequent and long excursions on asses, horses, and camels, and had spent much time in shooting and hunting, his game being chiefly waterfowl and wild boars. He remained two winters, from 1856 to 1858, and, imprudently, the intervening summer also in Cairo; he then made, in April, a journey to Jerusalem, from Alexandria to Jaffa in the steamer, from Jaffa to Jerusalem on horseback; a journey which cannot be performed without bodily fatigue. In the month of May following, I met him at Naples, where he consulted me professionally. He was, of course, worse. On accurate examination, I found the objective signs of infiltration and cavernous formation in the lungs, and the best marked signs of general phthisis. Exhausting attacks of diarrhœa had, moreover, supervened. Dreading the injurious effects of a southern summer in such a state of debility, particularly in chronic diarrhœa, I recommended him immediately to leave Naples, and either to return home, or to take a summer residence in an Alpine region. But, in spite of my frequent and urgent admonitions, the patient remained the greater part of the summer in Naples, made excursions, visited galleries and similar sights, and ate and drank whatever he fancied. Meanwhile he became steadily weaker. Finally, towards the end of July, he determined to leave, and called upon me in such a state, that I felt obliged to endeavour to dissuade him from taking any journey. I recommended him to stay quietly in Sorrento, as being cooler, Capri, or some similar neighbouring locality. But he was now as little to be kept back as he was before to be moved to take his departure. A true mortal agony seemed to urge him on. He set out in the steamer for Genoa. In embarking he had to be carried on deck. When the steamer returned to Naples, I inquired of the officials what had become of the unfortunate patient. I was informed that when the vessel arrived at Genoa, he was found dead in his berth. Neither can this case, on account of the absurd abuse of a southern residence, prove anything against the efficacy of the climate.

Though only few accurate and really useful reports of cases are to be found in medical literature, still we meet with some observations where the diagnosis, stage, and course of the disease, are scientifically given. And these observations are strongly in favour of the beneficial influence of a southern climate on chronic bronchial catarrh and chronic pulmonary tuberculosis. We find even cases described, where the subsequent post-mortem examination might

prove its favourable curative influence. Besides, general opinions and statements from competent sources, as to the efficacy of the southern climatic sanatoria exist, which likewise depend on empirical experience, without the particular and detailed observations being directly given. In such cases it is, of course, necessary to inquire into the competence of the persons giving the opinions, and only to admit such of the latter as are really reliable.

Another mode has been adopted of scientifically determining the importance of a southern residence for patients affected with pulmonary tuberculosis. Thus it has been attempted to make the geographical distribution of tubercle of the lung, the rarer or more frequent occurrence of the latter in the climatic sanatoria, a measure of the influence of the climate in question upon this disease. It is certain that the statistics of disease and mortality in a country ought to furnish important data for the medical estimation of a climate. But the statements which exist as to the relative mortality of those places must be used with circumspection. The medical statistics published for these localities have been freely used and taken as the groundwork of investigation. But they are neither reliable, nor do they admit of extensive conclusions; for, if we look at the hitherto political, social, but especially the medical circumstances of those places under consideration, we can expect no accurate and certain statistical figures. Moreover, these statistics do not comprehend the entire population, but usually only a small fraction of it—the inmates of hospitals for the poor, and the part of the inhabitants who are in general exposed to the more unfavourable conditions of life. But if we should admit that reliable and sufficient medical statistics existed, these would warrant only a conclusion as to the influence of the climatic conditions on the residents, who are exposed to them during the entire year. Now, when we send our patients to a southern climatic sanatorium, it is only for a sojourn during the winter season. The injurious effects of the southern summer do not reach them. Certainly, in modern times, when a southern residence has been extended over several winters, patients have been advised to remain in the south also during the summer, on account of the difficulty of getting home; but in that case the higher mountains of the countries in question, where the temperature is much cooler, have alone been considered adapted to the purpose.

Just as little do the statistical investigations instituted as to the occurrence and circumstances of chronic pulmonary tuberculosis in the French Navy\* prove anything against the climatic sanatoria. These investigations have shown that the proportion of those who die of that disease in the navy is to the other cases of death as 1 to 7.59, almost twice as unfavourable a result as in the land army, where this is as 1 to 13. Hence the practical rule has been

\* J. Rochard, *op. cit.*

deduced, to warn all young people who have a tendency to pulmonary tuberculosis, or who already present the incipient symptoms of the disease, against adopting the profession of a sailor. This conclusion may be justified. Considering that the stations of the French navy are, with very few exceptions, situated in a southern climate, it has been attempted to draw the further inference that a southern climate in general is injurious to those labouring under, or predisposed to, tuberculosis. But this inference is neither generally correct, nor does it prove anything against the so-called southern climatic sanatoria. For the sailor is exposed to many influences prejudicial to health, among which the many changes of weather, the limited space and crowded residence on shipboard, are to be enumerated. These and similar disadvantages press specially upon the calling of a seaman, but do not reach him who visits the south on account of his health. Moreover, the stations of the French navy lie in great part in the torrid zone, in the vicinity of the equator, in the East Indies, Africa, Senegal, the West Indies, South America, and in the Pacific Ocean; while the southern climatic sanatoria are in the temperate zone, on the shores of the Mediterranean Sea, in the neighbourhood of the latter, or in similar geographical situations. Finally, those investigations relate to the life of a sailor in a southern climate during the whole year, while a patient remains in a southern climatic sanatorium only during the winter.

While we must by no means infer that because pulmonary tuberculosis is of frequent occurrence among the inhabitants of a southern climatic sanatorium, the influence of the climate of the latter will necessarily be unfavourable to those patients who come thither from the north to spend a milder winter than they could enjoy at home; we may conclude, on the other hand, that the influence of the climate of those places where pulmonary tuberculosis is of rarer occurrence, will be beneficial to such invalids. Thus when, notwithstanding the unfavourable effects of the excessive heat of the southern summer, this disease is, in a certain locality, little met with, we must suppose that the conditions for the development of tubercle of the lung exist in absolutely less proportion, and that our patients must there meet with a climate so much the more favourable to them, as they are to spend in it only the mild winter. And there are, in fact, southern climatic sanatoria, where pulmonary tuberculosis is of rare occurrence. The places especially pointed out as such are Venice, Madeira, Algiers and Cairo\*. Moreover, for most of these climatic sanatoria experience shows that even if, with respect to some localities, it cannot be proved that pulmonary tuberculosis is of rare occurrence, the disease has a very protracted course among the inhabitants. But the most im-

\* See, for Venice, Sigmund's "*Südliche Klimat. Kurorte*," for Madeira, Mittermeier, *op. cit.*; for Algiers, Castalat, Bertherand, "*Gaz. Hebdom.*" 1858, p. 230, Haspel, "*Canstatt's Jahresbericht*," 1858, ii., 156; for Cairo, the writings of Pruner, Griesinger, Rayer.

portant empirical evidence of the suitability of a residence in the southern climatic sanatoria for patients affected with tubercle of the lung is that already mentioned—namely, that people who migrate from a southern to a northern climate are very likely to be attacked with pulmonary tuberculosis; while those who change from the north to a southern country rarely become tuberculous, and northerners so affected very frequently recover in the south. This is most strikingly seen in Egypt, where there is always a great confluence of northerners and southerners. The Arabs and Copts who have migrated from the southern parts of Egypt to Cairo, the Berbers of Nubia, and, above all, the Negroes of the south are, in Cairo, very frequently attacked with pulmonary tuberculosis; on the other hand, the Europeans who have migrated to Egypt are very rarely so affected; and those who have come from Europe to Egypt with a predisposition to the disease, or with well-marked symptoms of the same, are frequently improved or cured; indeed, it is said that Negroes, who have taken ill in Cairo, recover when they return to the south.

After these critical observations, I may be permitted to bring forward the positive results of the investigations as to the therapeutic importance of the southern climatic sanatoria, which appear to be best ascertained and best founded, and which, at the same time, present the greatest amount of practical interest.

Residence in the so-called southern climatic sanatoria may certainly be considered, in general, as a remedial measure in chronic bronchial catarrh and chronic pulmonary tuberculosis, for patients from the northern and central parts of Europe. On the other hand, it cannot be maintained that a southern climate is invariably, and under all circumstances, favourable to such patients. In the first place, tropical heat is often very destructive in its influence on the development and course of these diseases. Summer, too, in these sanatoria is, for the most part, injurious to the above-described patients. Only those localities can be considered as southern climatic sanatoria favourable to patients from our latitude, which are situated in the south of Europe, the north of Africa, or in similar geographical positions. These should, moreover, be used only as winter residences. According to our present climatological knowledge and medical experience, the following places are to be considered as the most favourable southern climatic sanatoria:—Malaga, Hyères, Nice, Venice, Pisa, Rome, Palermo, Cairo, Algiers, Madeira. After these, because less known and less recognised as sanatoria, are to be named: Cannes, Villafranca, Mentone, San Remo, Naples, Messina, Catania.

The efficacy of a residence in the southern climatic sanatoria is to be ascribed specially to the greater warmth of the southern winter. By removing a patient labouring under pulmonary disease to one of those southern localities a double advantage is obtained—first, the negative, that the patient escapes the injurious influences of the

northern winter, whereby his lungs and constitution are afforded an opportunity of recovering; secondly, the positive and direct, arising from the frequent enjoyment of a mild and pure atmosphere, and the power of taking bodily exercise, advantageous alike to the respiration and to the nutrition of the body.

Besides the greater warmth, other climatic elements also contribute to the cure, especially a certain regularity and limitation of the alternations of temperature, the purity and moderate movement of the air, as well as the rarity of rain. The amount of moisture in the atmosphere may likewise play an important part; but it cannot be truly asserted that a definite degree of atmospheric moisture is particularly favourable in all cases of these pulmonary diseases; it is rather to be assumed that, in some instances, a moister, in others a dry, climate is advantageous. What cases are better suited to one climate or the other, we have as yet no certain indications to show; but from what has been practically and theoretically established with respect to the several sanatoria, we may consider a moist, moderately warm climate to be, in general, a soothing and gentle expectorant in chronic bronchial catarrh and chronic pulmonary tuberculosis; and a dry, moderately warm climate to be secretion-limiting, tonic, gently-exciting, and specially promotive of the general nutrition. The former would, therefore, be more suitable to cases of these diseases accompanied by a sensitive state of the bronchial mucous membrane, viscid expectoration, frequent and dry cough, and an easily excitable condition of the vascular and nervous systems: the latter to the torpid form of the disease, with abundant expectoration, and to patients in whom the principal indication is to strengthen the system at large.

The several climatic agents may be grouped in different ways, may cooperate or counterbalance one another, and so, in various modes, contribute to the cure.

Besides the climatic, other influences are to be considered, deducible from change of locality, social relations, occupation, and the whole mental and bodily mode of life. These influences, it is true, possess only an individual importance; but they may, under some circumstances, play a very important part.

In conclusion, I shall collate the southern climatic sanatoria whose climates are best understood, according to their mean winter temperature and their winter conditions of moisture and rain, placing them so as to form ascending series.

N.B.—The following numbers are the results of mean calculations based upon observations continued, in the majority of instances, during many years, and are borrowed from the monographs upon these sanatoria already quoted—Mittermeier's upon Madeira, Reil's on Egypt, Sigmund's on the sanatoria of Northern Italy, and especially Vivenot's on Palermo, in which latter work the most numerous and most complete tables are to be found. The position in the series, when it is not justified by numbers, is based upon the



explicit opinions of these and other competent authors (Bertherand on Algiers, Francis\* on Malaga).

### I. SERIES,

Exhibiting the mean temperature of the winter—that is, of the months of November, December, January, February, and March:—

Venice, (40°.59F.).	Naples, (51°.53F.)
Hyères, —	Palermo, (54°.29F.)
Florence, —	Catania, (54°.83F.)
Pisa, (45°.79F.).	Cairo, (58°.64F.).
Nice, (47°.12F.).	Malaga, (59°.F.)
Rome, (48°.62F.).	Algiers, (61°.06F.).
Cannes, —	Madeira, (61°.47F.).
San Remo, —	(Funchal)
Mentone, (50°.0F.).	

In the case of Malaga and Algiers, the temperature is calculated for the winter, including October.

### II. SERIES,

Representing the mean atmospheric moisture in winter:—

1. Cairo.	6. Mentone.	} Mean for Nov., Dec., Jan., Feb., and March.
2. Algiers.	7. San Remo.	
3. Hyères.	8. Naples.	
4. Nice.	9. Rome.	
5. Cannes.	10. Pisa.	
11. Madeira, 75.4°/o. (Funchal).		
12. Palermo. 76.2°/o.		
13. Venice, 80.4°/o.		

### III. SERIES,

Showing the number of rainy days in winter—that is in November, December, January, February, and March:—

Cairo, 13.7. See Series IV.	Madeira, 55.7.
Malaga, —	(Funchal).
Nice, 25.7.	Florence, 57.4.
Venice, 28.1.	Rome, 58.7.
Algiers, —	Pisa, —
Catania, 45.3.	Palermo, 65.7.

\* [A review of Dr. Francis's work on Change of Climate may be found in the sixteenth volume of the present series of this Journal, August, 1853, page 147.—  
TRANSLATOR.

## IV. SERIES,

Arranged according to the amount of rain in winter—that is, in November, December, January, February, and March:—

Cairo,	scarcely measurable.	Naples,	413.90 millimetres,	
Venice,	229.99 millimetres.	Madeira,	548.44	„
Palermo,	364.06	„	(Funchal).	„
Rome,	407.94	„	Pisa,	548.90

*On the Treatment, without excision\*, of Wens and of some other Cysts.*  
By M. A. COURTY, Professor in the University of Montpellier.

To speak of wens is to touch upon one of the most humble and common subjects in surgery; for there is no application of our art more frequent, and in some measure more routine, than the treatment of those tumours, often so numerous, and of dimensions so varied, which are developed on the head, in the substance of the scalp, or below it, in the neck, on the face, and some other parts of the body, consisting of a cyst and its contents, whatever may be the variable thickness and density of the envelope, the solidity or fluidity of the encysted matter.

Removal is a prompt and easy method of dealing with them; but in addition to the fact that it is not always exempt from accident, it is not readily consented to by patients. Not to mention the numerous cases in which the multiplicity of the wens makes the patients dread the renewal of the pain they have once unwillingly consented to undergo, these tumours being naturally painless, often little troublesome, not interfering with any function, not threatening life, nor causing imminent danger nor real infirmity, it is natural that they should be borne, until their great development, inducing the inconvenience of a disagreeable deformity, or the danger of approaching degeneration, overcomes the timidity of the patients, and hastens their determination to submit to operation. It still remains for us to combat the prejudice entertained, that in touching a wen we may run great danger, not only of such local accidents, as erysipelas, inflammation, or epithelial development, which are real; but still further by the possibility of supposed metastases, or of some disturbance which their removal may excite throughout the system.

These considerations have induced me to say a few words on the development of these tumours, to show that it might always be prevented, if advice were sought in time, and to describe the mode of treatment which I have found most successful.

\* Men who have attained to eminence in the science of medicine have often been reproached with neglecting the discussion of common, though important subjects. The proverb, "*De minimis non curat prætor*," shows that this reproof is of ancient date. Professor Courty appears anxious to make an exception to the rule; in this he sets a good example: we, therefore, hasten to reproduce his essay on the Treatment of Wens by Cauterization.—ED. B. G. de T.

Under the name of wens are included subcutaneous, indolent, circumscribed, encysted movable tumours, susceptible for the most part of acquiring a considerable volume, and containing a fatty or oily matter. Cyst and fatty matter are the two terms, the union of which may be regarded as characteristic of every wen (*loupe*). As fatty cystic tumours, wens must be distinguished anatomically, physiologically, and surgically—on the one hand, from other fatty tumours; on the opposite, from other cystic tumours.

The other fatty tumours are lipomas, true local hypertrophy of the adipose tissue, capable of producing equally all the elements of that tissue, or only some of them, or some more prominently than others, sometimes complicated with the addition of new fibro-plastic productions, or by the coexistence of crystallization of fatty matters (cholesteatoma). They ought to be called fatty, rather than fat, tumours. Far from being confined in a cyst, they are usually movable, ill-defined, insinuating themselves into the interstices of the tissues; and unless they are pediculated, having generally no boundaries but the aponeurotic envelopes which separate the different planes, or which surround an organ. They present a tuberculated aspect, prolongations, supposed roots, a characteristic softness, a deceptive fluctuation. Thus they require dissection, the only method which enables us to remove them totally, and to avoid the dangers inherent in other modes, of leaving behind portions, or of promoting the formation of purulent sinuses; and sometimes the source of the adipose tissue is at a distance more or less great from the seat of cauterization, of the seton or the ligature. We are sometimes astonished at the distance to which we are led by dissection to follow the excentric lobes of lipomas between different muscular planes; I have removed some on the chest which insinuated themselves under the great pectoral. A few months ago I removed an enormous lipoma, the prolongations of which I had to follow to the deepest plane of the abdominal muscles. I have lately seen another, insinuating itself under the trapezius and rhomboid, as far as the spinal muscles.

The other cystic tumours may depend, like wens, on the dilatation of a cavity accidentally or naturally closed, by a normal or abnormal liquid secreted in this cavity (mucous cyst, hygroma, ovarian cyst, ranula, &c.); but they may depend also on the formation of an envelope around a sanguineous extravasation, a foreign body, an entozoon, or a cancerous formation. In a word, there are between the containing and the contained variable relations of preformation and postformation, which affect not less than the nature itself of the tumour, the determination of the indications to be fulfilled to accomplish a cure.

As to wens, whatever be their dimensions, there is, in the contained and the containing substance, a constancy of composition and of origin, which makes a perfectly natural order of tumours, the order of sebaceous tumours or cysts.

The containing organ, that is to say, the cyst, is formed by the thick walls of the simple sebaceous clustered glands, which are the starting point of the development of the wen. It therefore exists previously to this development, and differs from itself, in its different periods, only in the transformation it has undergone. The orifice of the excretory canal becomes obliterated, or remains insufficient for the evacuation of the superabundant products of a vitiated secretion; it may even successively reopen and be obliterated several times, permitting the cyst to be alternately collapsed and distended, to empty and refill itself anew. The surgeon, when he discovers this orifice, usually recognisable by the existence of a black point, may make use of it for the evacuation of the contents, and for the ulterior treatment of the cyst by the method I am about to explain.

The contents, more or less liquid, sometimes solid, suety, or chalky, are formed of sebaceous matter, that is to say, of free fatty granulations, globules of various sizes, of little drops, or of an oily homogeneous liquid, of cells of pavement epithelium, often without nuclei, with or without granulations, often altered, plaited, shrivelled, some of them vesicular, ovoid, distended by their contents and detached from the inner surface of the gland; lastly, of crystals of cholesterine and of granulations, or of a sort of mortar of carbonate of lime or magnesia. In one degree, the tumour, known under the name of grub (*tanne*), is still small, and is filled simply with thickened sebaceous matter. In a second degree, it, more or less voluminous, includes sometimes a yellowish matter, having the consistence of honey (*mélécéris*), sometimes a whitish substance, occasionally resembling thick pus or pap (*atheroma*), which, from its flat form, or from some other circumstance, has received the names of *talpa*, *testudo*, &c. In a third degree, the tumour, very large, is formed by the accumulation of a fatty substance, having the consistence and colour of suet (*steatoma*); it is often accompanied with hypertrophy of the gland, with a tendency to the formation of an adenoid tumour, with a disposition to epithelioma, justifying the opinion of those surgeons who have pointed out the possibility of the cancerous degeneration of steatomatous tumours.

The precision of our knowledge of the nature of wens scarcely admits of our discussing the questions of the danger which may accompany their cure, of the possibility of a metastasis after their suppression, &c. It is to a certain point the same with respect to the treatment to which it has been proposed to submit patients, with a view to combat the lupial or wenny diathesis. Is there only diathesis? It is easy to answer this question in the affirmative, if we take diathesis in the sense of predisposition; for it is certain that in some individuals there is very great tendency to obliteration of the excretory canals of the sebaceous glands, to superabundance, alteration, thickening of their secretion, and consequently to the development of wens; but can we regard as a permanent morbid affection, what is, in fact, only a more or less frequent effect of the retention of secreted products?

On the other hand, the accuracy of our knowledge enables us to adopt the best surgical treatment of these tumours. Setting aside removal, which must sometimes be practised, it may be said that the destruction of the cyst is the key to every method, the application of which is capable of producing a permanent result. It remains to determine the best mode of destroying the cyst, and the cases to which the method is applicable.

Let me say a few words, first, on the removal of the tumour by means of cutting instruments.

This operation is usually indicated in cases of very voluminous wens, although, to say the truth, it may be dispensed with even in such cases. It is, on the other hand, more formally indicated for little cystic tumours of the eyelid,—being, moreover, in this particular instance, perfectly harmless. Lastly, it is applicable to wens of medium size, and may be performed with such rapidity as to give the patient scarcely any pain. To make an incision in the skin, and rapidly enucleate the cyst with a spatula, as has been so dexterously done by Dupuytren; or, after having divided the wen through the middle to its full extent, to remove, one after the other, the two halves of the cyst, seized with a strong forceps, as M. A. Guérin does, is to combine the certainty of success with the greatest operative rapidity.

I have often operated in this mode, and have always had to congratulate myself on having done so. Lately I removed by this process, in a young lady, and some days after in a man, two atheromas as large as nuts, situated in the lateral region of the neck, without giving the patients time to utter a complaint. The wound subsequently united, also, with great rapidity. It is, however, well known that the consequences of an operation, apparently so slight, may be aggravated by the development of erysipelatous inflammation, and even of phlegmonous erysipelas. We know that these accidents may be serious, especially when the dimensions of the cyst, its adhesion to the integuments, the thinning of the skin, oblige us to practise a more or less laborious dissection. We know, in fact, as I have already stated, that patients often refuse to submit to this mode of treatment.

These several reasons have led me to perfect the plan of proceeding by destruction of the cyst.

I shall begin by observing that the destruction of the cyst does not necessarily involve that of the skin. It is well to make this remark, because the majority of surgeons who have applied agents of destruction, especially of cauterization, in the treatment of wens, have applied them on the skin; differing not much in that from quacks, who, by means of so-called dissolving plasters, or of pretended chemical pencils, determine on the skin, throughout a variable extent, the painful formation of eschars more or less extensive, which may become, as well as incision, the source of inflammatory symptoms. Whether, with M. Legrand, we employ linear cauterization with caustic potash, or, with Bonnet, superficial cauteriza-

tion with Vienna paste, or pâte de Canquoin, or, with M. Maison-neuve, *cautérisation en flèche*, &c., the skin is always attacked. I have had occasion to see this cauterization followed by erysipelas; and even when the bottom of the cyst was thus exposed, without being destroyed like the surface, I have witnessed an epithelial vegetating production developed at the bottom, and soon assuming the aspect of an epithelioma, or of a cancrroid, little encouraging to the operator, and necessitating a fresh and extensive application of the caustic.

I, therefore, lay it down as a principle that the skin ought to be, so far as possible, preserved, and that the destructive agent ought to be carried into the very cavity of the cyst.

In what mode are we to penetrate into the cyst?

Almost always in grubs and little wens, often even in voluminous tumours, we find a black spot indicating the seat of the neck of the orifice, which is supposed to be necessarily obliterated, and which is often only plugged. Nothing is easier than to remove the obstruction by pressure, or by the action of some very simple chemical or mechanical agent, a little soap and water, alkaline solution, the point of a probe, of a pin, or needle. We empty the cyst by pressing it; sometimes the thickened matter comes out gradually, in the form of a worm; sometimes, more fluid, it escapes in a jet, and is quickly and entirely expelled by the pressure of the fingers.

If this orifice is deficient, the point of a needle, of a little trocar, readily and painlessly substitutes for it an artificial opening.

Finally, if the cyst is very voluminous, if it is multiple, as I have seen in several instances, a double puncture, kept up by the presence of a seton during a few days, suffices to afford access to a single or multilocular cavity, and to give time and opportunity to express by degrees the entire contents, without exciting pain, or giving rise to any inflammatory symptom. To make this double puncture, and introduce the seton, I prefer the trocar to the needle, the more rapid and less painful introduction of which forms openings larger, and less sensible to the contact of the thread. I then perforate the tumour through and through with a trocar, leaving the canula some moments *in situ*. Into this I pass the seton, which, therefore, does not touch the openings; then I withdraw the canula, and fix the seton by attaching its two extremities to each other. When the tumour is very voluminous (and I have been able thus to operate on some which were as large as a turkey-egg, or as an orange), I make use of M. Chassaignac's drainage trocar, the point of which, furnished with a notch to which the thread may be attached, allows the tumour to be perforated, the seton to be passed into the canula, and the latter to be withdrawn in less time than it takes to describe this little manœuvre.

The cyst having been emptied, how must we proceed to effect the obliteration of its cavity?

Strictly speaking, we may have recourse to one of two modes:

either to modify its inner surface, to irritate it, inflame it, and determine by approximation and contact the adhesion of the opposite walls; or to produce its more or less rapid destruction, and its expulsion by the orifice, the cutaneous envelope quickly retracting, and gradually filling up, either by retraction or by deep adhesions, the void caused below it by the expulsion of the cyst. This latter mode is far preferable to the former.

At first, that is to say, about fifteen years ago, I was content to aim at simple modification of the walls of the cyst. To this end, after having expelled all the contents, I injected into the cavity various liquids. I had adopted, in preference to all others, a strong solution of caustic potash (one part, and sometimes more, to fifty parts of water), thrown into the cavity by means of a small glass syringe. This solution had the advantage of first dissolving all the fatty or sebaceous matter which remained adherent to the walls, and then of irritating the latter, and disposing them to adhesion. Methodical pressure, continued or interrupted, favoured by the natural tendency to retraction of the skin and of the cyst, which were no longer distended by their contents, brought about by degrees the adhesion of the several portions of the sac, which eventually came into mutual contact, and the radical cure was finally attained after the lapse of a period varying from about fifteen to fifty days. On no occasion did pain or any other accident interfere with the treatment.

Since that time, thanks to the constant progress of cauterization applied to surgical operations, I gradually substituted for the caustic potash—of which I no longer made use, except to completely wash out the sac—agents more energetic, and more easily managed. Canquoin's paste, and particularly his various sparadraps, such as the school of Lyons has introduced into ordinary use, appeared to me perfectly to fulfil the object I proposed, and I have not ceased to employ them in the treatment of wens.

The following is the mode of proceeding:—A piece of sparadrap of chloride of zinc is cut of a length proportioned to the capacity of the cyst, narrow enough to pass through the opening; its penetration may be facilitated by rolling it into a cylinder, so that the caustic substance occupies the external surface of the long and slender cylinder. It is always easy to push it into the interior, the natural orifice of the wen admitting of its passage with the slight aid of a director, a probe, or the head of a pin; the artificial opening, if it exists, being so much the more convenient, as it must have, as we make it, more considerable dimensions. For grubs, a piece of Canquoin as small as a grain of millet, or a lentil, is sufficient; for the more voluminous wens, a cylinder from one to three centimetres in length will be required. If the cutaneous opening appears insufficient, the end of the caustic is retained in it, instead of being forced entirely into the cavity of the cyst.

Pressure exercised on the wen at least twice a day gives exit,

from this time, to a purulent matter, occasionally accompanied with small shreds of the cyst, and remains of the caustic sparadrap. But here two cases present themselves: either the cyst is small, very movable under the skin which covers it; or it is very large, and adherent to the tissue in a great number of points.

In the first case, that is to say, when it does not exceed the size of a large nut, which is not so very small, one cauterization is often sufficient to effect a cure. Whether this takes place after one or several introductions of the caustic, it is usually accompanied with the expulsion of the modified cyst. By the use of pressure, the latter makes its appearance at the opening, whence it is easy to complete its extraction with the forceps, when its expulsion does not occur spontaneously. We then obtain the cystic sac intact, the dead fibrous tissues presenting a whitish or gelatinous appearance. It is unnecessary to say that the complete obliteration of the cavity, the retraction of the skin, and its adhesion to the deep-seated parts, soon follow upon the expulsion of the sac.

If patients were well-informed as to the facility of freeing them without pain or danger from these grubs and little atheromas, which threaten to become large wens, we might with certainty prevent the development of every tumour of this kind. To introduce through the natural opening of the grub, or through an artificial opening made with a needle, after having evacuated its contents, a small portion of caustic of the size of a lentil, and thus to destroy the cyst, the expulsion of which takes place almost spontaneously, would be a matter of a few days. We should thus always prevent a small, indolent tumour from ever becoming an inconvenient or painful deformity; and we should avoid exposing the patient to the sufferings and dangers of an operation, which at last becomes indispensable.

In the second case, that is to say, when the cyst is voluminous, of long standing, multilocular, &c., we cannot hope for a termination so rapid or so complete. We must then reckon on the successive expulsion of shreds of the cyst, which gradually mortify, or on the adhesion of the points of the sac which shoot out sufficiently, in consequence of the irritating action of the caustic, to unite one to another. The successive introduction, at some days' interval, of several pieces of Canquoin's sparadrap, methodical pressure exercised on the wen either continuously or interruptedly, care to keep it always emptied of the products secreted in it, and to favour the retraction of the skin, and the contact of the opposite surfaces, finally bring about by degrees a result as advantageous as that obtained in the first case at a less expenditure of time and money.

The duration of the treatment may then vary from thirty to fifty days, but the result is always cure; and the patients attain this end without having incurred the least danger, or experienced the slightest pain. Contrary to what is true of the skin, the inner surface of the cyst has always appeared to me to be insensible to the action of caustics, all the benefit of which is consequently obtained,



without our having to fear the reactions which follow the development of the pain inevitably connected with their application to the cuticle.

I have never observed, in the very great number of wens I have in this mode operated on, the production of a single one of the accidents which may follow any surgical operation.

The large number of cases which I should have been able to quote in support of the efficacy and harmlessness of my method of treating wens, the uniformity and monotony of these details, have decided me to bring forward none, each particular observation having been the exact reproduction of the general history I have just traced.

It is scarcely necessary to add, that certain mucous cysts, slightly-developed hygromas, &c., may be treated as I have done, with as much success, by the same method.—*Bulletin Général de Thérapeutique*, 15th June, 1860, page 490.

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*On the Local Action of Savin.* By DR. EISENMANN, of Wurzburg.

THE Hungarian Journal of Physiology and Medicine, 1859, No. 5, and from it the Medico-Chirurgical Monthly Papers for May, 1859, contain a communication by Dr. Moller, entitled, "Savin as a Popular Remedy," according to which it would appear that in Hungary a decoction of savin is in common use for the removal of polypi. The author is acquainted with the case of a peasant woman, who by means of this agent got rid of a nasal polypus, and he himself successfully employed it to destroy and remove a soft nasal polypus which he had twice in vain extracted. On account of the softness of the polypus, he had not been able to extract the root, and it had consequently always again grown rapidly. The following is the mode of using it:—A drachm of savin tops was boiled for five minutes in about three or four ounces of water, and the polypus was painted frequently during the day with the filtered decoction; the fluid was also injected into the nose. This communication reminds me of some results I have myself observed from savin. In Hecker's old *Materia Medica*, the following preparation is found under the name of Hecker's salve. An onion is roasted in the ashes; the juice is expressed, and with this juice and a corresponding quantity of powdered savin, a sort of liniment is prepared. This liniment is recommended by Hecker in the treatment of condylomata, and in fact it removes venereal warts in a surprisingly short time; during twenty years' experience I repeatedly observed very fully developed condylomata disappear completely after three days' use of this ointment. These remarks may serve to introduce the following.

During my stay at the fortress of Oberhaus, at Passau, I became acquainted with Bonkratz, the drummer of the battalion, who attracted my attention in consequence of a tumour he had on his neck, beneath the right ear. This tumour was circular and flat, was equal

in circumference to a crown-piece, rather over than under it, and was about a quarter of an inch in thickness. It had a tolerably wide base, was hard as fibro-cartilage, and presented a bluish red appearance. This tumour had been frequently removed with the knife, and its root extirpated by the actual cautery, but it had always grown quickly again. In other respects Bonkratz was in good health, had no traces of syphilis, and his skin was free from eruptions and ulcers. The causes of the tumour were quite unknown. I spoke to Dr. Delzer, at that time surgeon to the regiment, about the case, mentioned my observations on the efficacy of savin in the treatment of condylomata, and suggested to him to make a trial of Hecker's salve with this swelling, which resisted all other remedies. Dr. Delzer, whom I always found extremely friendly, agreed to my proposal, and promised to carry it out. However, in September, 1843, I left the fortress, and lost sight and recollection of Bonkratz. In the autumn of 1847 I met the same Bonkratz at Nürnberg, where he had been employed at the custom-house, quite unexpectedly in the street. The tumour in his neck was completely gone, and, as he said, in consequence of the use of the remedy I had recommended.

Savin, therefore, exercises a remarkably deleterious influence on condylomata, polypi, and tumours, which may perhaps be regarded as polypi of the external skin, and who knows how much further this influence may extend?—*Virchow's Archiv*, Band xviii., 1 and 2 Heft, page 171.

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*On the Influence of Warm Climates on Phthisis.* By PROFESSOR  
FORGET, of Strasburg.

THE question of climates as causes of certain diseases, immediately suggests the idea of their curative power with respect to other affections. It is, in fact, generally admitted that the diseases produced by cold, being rarer in hot countries, ought also to be cured by residence in hot countries; the converse is not equally true, for atmospheric cold produces many more diseases than it can cure. However, it is admitted that dysentery, for example, produced in hot countries, improves under the influence of a climate not cold, but temperate.

According to these principles, it was, until latterly, universally admitted, that the diseases of the chest produced by cold, phthisis in particular, were favourably modified by residence in hot countries; but the love of innovation, and the rage for the paradoxical, which have seized upon the medical heads of our epoch, have succeeded in changing all this; and the Academy of Sciences has just crowned a work, the conclusion of which is, that a sojourn in warm climates favours the production and development of phthisis. The author goes further, he explains why heat is prejudicial to tuberculous patients: it is, he says, because it engenders the debility which itself presides over the generation of phthisis. Behold, then,

the question judged by a formidable array of statistical documents, which seems to leave no room for discussion.

Well! I declare this to be a gross illusion—I do not say an error; for, even admitting the fact, which is not fully proved, I undertake to explain the most rudimentary according to the simple laws of pathology. This is not a quibble; for in admitting, *in globo*, that hot countries are fatal to phthisical patients, you take from the latter all desire to go seek relief under a milder sky than ours; while, by learnedly interpreting the pernicious effects of hot countries, you teach patients to preserve themselves from these effects by profiting by a salutary climateric influence.

And, in the first place, to return to our starting point, it is, *à priori*, extremely probable that phthisis produced by cold ought to be relieved by heat. So says common sense.

Let us now consult experience. In like manner as patients set little value on simple and indigenous remedies, and place their confidence in those that are expensive, physicians affect a decided disdain for natural scientific processes, within the reach of all understandings, and take pride in searching in darkness for truths which strain their eyes. This eccentricity of mind has other springs than simple pedantry; and, among other causes, it results from the horror of rationalism implanted in the minds of the rising generation during the last thirty years. Instead of investigating with so much labour documents borrowed from the English records in both the Indies, instead of sending at a great expense experts to Africa or elsewhere, it would have sufficed, I think, and would have been right, first of all to examine what passes around us.

It has been justly said that seasons are transitory climates; and every one knows that our summers often present a temperature equal to that of inter-tropical climates. In fact, even the equatorial heat is difficult to bear by its continuity, rather than by its excess. Well, how does phthisis go on during summer? And, first, the fact is that unfortunate consumptive people dread cold, dread winter, and justly so, and they expect the return of warmth as the convict does his pardon. As soon as the vernal sun shines forth, they believe they may take their fling, and expose themselves to those variations of temperature which render that season so fatal. It is only in summer that they breathe at leisure, and in security; but on one condition, however, that they avoid the influence of a too burning sun. This is because extreme heat is an irritant; and, when consulted by consumptive patients, I never fail to give this injunction: Avoid cold, and *extreme heat*. Great heat stimulates the bronchi; simple catarrhal patients suffer from it like those that are consumptive, and tell you that an evening spent in too hot an apartment occasions them bad nights, that is to say, gives rise to coughs, oppression, fever, &c. A summer catarrh has recently been described; all practitioners have observed it; it is common among nervous people, *with delicate chests*; but it does not resemble the winter catarrh—it

is less humid; it is, in general, characterized by a dry nervous cough; the oppression, the quickness of the pulse, indicate irritation and congestion, rather than inflammation of the air-passages.

But if, instead of braving the sun, of seeking it, even, as a means of relief, patients mistrust it, keep in the shade without moving about, in an apartment protected against the invasion of external heat—if they choose the early and late hours of the sun's course to take their exercise—they then reap all the benefits of the warm season, without experiencing its inconveniences.

Well! apply these precepts to life in the colonies; impose upon patients the indolent habits of the Creoles of the Antilles, or of the Nabobs of India, and you will be obliged to deduct much from your lists of mortality. Instead of that, what have you done? You have taken your subjects of observation, and of statistics, from among the poor soldiers, or unfortunate sailors obliged to keep guard, or to work hard, under a vertical sun, and given the results as proofs of the pernicious influence of hot countries upon phthysical patients.

I may add, that navigators have pointed out the rarity of diseases of the chest within the tropics, which is also a legitimate corroboration of my proposition.

It is, therefore, not by the weakness they produce, that hot climates favour phthisis; it is, on the contrary, by the stimulation which extreme heat exercises on the respiratory passages when chronically affected. And in this, these climates are neither more nor less pernicious than our European summers; and if you come to an unfavourable conclusion with respect to the first, you must come to a similar conclusion with respect to the latter,—that is to say, you must announce an absurd proposition, against which the patients themselves will be the first to protest.

*Conclusion.*—Hot climates are unfavourable to phthysical patients only through the excess of solar heat. But by moderating this excess by obvious means, we obtain from a sojourn in hot climates the most happy results, confirmed by the testimony of numerous patients, who owe to emigration the prolongation of life, and occasionally perfect cure. It is this which renders residence in the neighbourhood of the tropics more favourable than under the equator, and which makes it better to live at Madeira, or in Northern Africa, than in Bengal or Brazil. And the reason why residence in the South of France is little favourable to phthysical patients is, that winter there still makes its rigour felt, against which the inhabitants neglect too much to fortify themselves.—*Gazette Hebdomadaire de Médecine et de Chirurgie*, June 8, 1860, p. 376.

*On the Diagnosis of Diseases of the Heart.* By Professor SKODA.  
Reported by B. ABELLES.

*Pericardial Murmurs* are distinguished from endocardial: 1, by the character of slight contact and crepitus, while endocardial murmurs are blowing, grating, or moaning; 2, by their non-coincidence with the systole or diastole (drawling of the murmur); and 3, by the fact that these murmurs are heard only in a limited space, and not unfrequently vary with the position of the patient. *Endocardial murmurs* exhibit exactly the reverse; and their distinctness *not unfrequently* depends on the rapidity of the contractions of the heart, those which are caused by constriction of the ostium venosum becoming plainer when the heart's action is slower, while those produced by insufficiency become more distinct when it is more rapid. The diagnostic interpretation of murmurs arising within the heart will be possible only by considering the results due to a decided valvular lesion. Both insufficiency and constriction will, on account of the reflux of blood into the auricle, be attended with a diminished flow of blood into the aorta, and therefore with retardation of the circulation. The increased pressure of blood will overcome the resistance caused by the contractility and elasticity of the walls of the auricle, and so effect the dilatation of the latter, which will extend from it to its veins, and thence to the corresponding arteries, and finally even to the ventricle whence the artery arises. The permanent overfilling of these parts gradually leads, by increasing the labour of the ventricle, to hypertrophy of the auricle and ventricle, while in the parts of the heart not affected by the valvular lesion the reverse is the case. In insufficiency or constriction of the left ostium venosum, the circulation will consequently be feeble in the aortic system, while the hypertrophic right ventricle overfills the pulmonary vessels. The two factors, retardation of the circulation and the abnormal pressure in particular organs, will act injuriously on the nutrition of the system at large; and the cyanosis and dropsical effusions of patients labouring under heart disease are to be attributed not so much to the predominatingly venous or hydremic quality of the whole mass of the blood, as to congestion of the venous circulation; hence, too, may almost all disturbances of the functions of particular organs be explained, which so very much promote the sinking of the patient. Thus the various pulmonary affections are explained by overfilling of the pulmonary circulation; congestions of the *venæ portæ* and cava lead to intestinal catarrh, constipation, enlargements of the hemorrhoidal veins and granulation of the liver, congestion of the peritoneal veins to ascites. The same is true of the occurrence of *Morbus Brightii* and of the several abnormal phenomena connected with the nervous system. With reference to the distinctness of murmurs, the principle obtains that the murmur in general originates where it is most loudly heard. The question whether a murmur belongs

to the systole or diastole, is decided by ascertaining whether it corresponds in time with the pulse of an artery near the heart. Sounds and murmurs which coincide with the pulse or somewhat precede it belong to the systole, while those following the pulse are due to the diastole. The systolic heart-sound is best heard at the apex, the diastolic, on the contrary, somewhat higher and to the left, which phenomenon depends on the change of situation of the heart in passing from the systole to the diastole. The sounds of the right ventricle are heard in the middle of the lower half of the sternum; those of the pulmonary arteries in the third intercostal space on the left side, half or a full inch from the edge of the sternum; the sounds of the aorta are most audible at the insertion of the third left or right rib. Systolic murmurs indicate insufficiency—diastolic, constriction of the orifices of the heart, while in the aorta and pulmonary artery the opposite obtains.—*Vierteljahrsschrift für die praktische Heilkunde*, 1861, Band 69. *Analekten*, p. 43.

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*On Hematuria.* By Professor OPPOLZER.

IN hematuria it is often difficult to determine whether the blood comes from the bladder or the kidney, inasmuch as, for example, we cannot with certainty infer the existence of renal disease from the presence of exudation-cylinders which microscopically exhibit the form of Bellini's tubes, for ordinary blood-coagula may accidentally present this form; therefore renal disease is to be assumed to be the cause of the hemorrhage only when the exudation-cylinders contain but few blood-corpuscles on their surface. The rapidity of the deposition of the sediment in the urine is by no means decisive in favour of vesical hemorrhage, as the duration of the contact of the urine with the blood is not known. The author considers the full investigation of the urine alone to approximate to a solution of the question. This in renal diseases exhibits decrease of the urea, uric acid, and colouring matters (particularly of Helier's urophaëin), increase of the urinary indigo, and an almost constant accession of albumen. If, on the other hand, symptoms of a vesical affection are observed, there is the possibility of vesical hemorrhage, especially if the urine comes in a decomposed state from the bladder. The causes of vesical hemorrhages are manifold: stagnation of the urine, spasm of the bladder, paralysis, injuries of the vesical region and of the bladder by foreign bodies (calculi), cancer; but certain medicines (cantharides) and changes of the system at large (scurvy) may also give rise to bloody urine. The same thing, moreover, happens spontaneously, as in the inhabitants of the Isles of France and Bourbon, or it is, as in Egypt, also caused by the hematobium, which may also give the impulse to the formation of urinary calculi. In all these cases the seat of the hemorrhage must be deduced from the symptomatology of the anomaly in question. Renal hemorrhages occur in Morbus

Brightii; and inflammations of the kidney are connected with renal calculi, cancer, and tubercle of the kidney, but are observed also in cardiac diseases, compression and obturation of the renal veins, in scurvy, and strongylus gigas. The treatment of hematuria consists in the treatment of the disease on which it depends.—*Vierteljahrsschrift für die praktische Heilkunde*, 1861, Band 69. Analekten, p. 62.

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